Long-Term Impacts of Primary School Scholarships: Evidence from Cambodia

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Abstract

This randomized trial investigates the long-term effects of a primary school scholarship program in rural Cambodia. We estimate impacts—nine years after program inception—on educational attainment, cognitive skills, socio-emotional outcomes, labor market outcomes, and well-being. Our results point to systematic improvements in educational attainment but no average impacts on long-term cognitive or socio-emotional outcomes. A merit-based (as opposed to poverty-based) targeting strategy did, however, increase cognitive outcomes, especially for poorer students. The results suggest positive effects on cognition for males. We find no improvements in labor market outcomes, yet positive effects on well-being, driven by recipients of merit-based scholarships. These findings shed light on the complex relationship between barriers to primary schooling and long-term outcomes, emphasizing the need for targeted approaches that consider both socioeconomic factors and individual merit, while also raising important questions about gender dynamics.

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1. Introduction

How does additional schooling relate to long-term life outcomes? According to the canonical human capital model, labor markets remunerate the skills acquired during the education process (Becker 2009). According to a signaling model (Arrow 1973; Spence 1973), education provides the market with a signal of individuals' higher abilities; as a result, the market pays for these skills. Both models predict positive effects from investment in education. At the same time, emerging research is showing that, in many settings, increased schooling has not meant increased learning, which potentially limits the market returns to education (Pritchett 2013; The World Bank 2018). However, there are few empirical studies in low- and middle-income countries that isolate the causal impacts of schooling on skills accumulation in the long run (Bouguen et al. 2019; Molina Millán et al. 2019). Our aim is to contribute to this nascent literature in developing countries.[1]

In this article, we present the causal long-term impacts of a scholarship program on cognitive skills, socio-emotional outcomes, labor market outcomes, and socioeconomic status and well-being[2], in a group of, on average, 21-year-old individuals who were offered the scholarship nine years earlier, in Cambodia.[3] A first follow-up study, three years after the program's inception, showed two main effects: higher school progression for individuals receiving the scholarship (compared with non-recipients) and impacts on cognitive skills only for those receiving the scholarship under a merit-based (rather than needs-based) targeting scheme (Author 1 and Author 3 2016). In this paper, we report impacts on long-term outcomes from data collected in late-2016/early-2017—nine years after the beginning of the

[1]Evaluations of Conditional Cash Transfer (CCT) or scholarship programs launched during the 1990s and the early 2000s in low- and medium-income countries are now allowing the exploration of such long-term effects of early-life interventions. The effects of these programs in the short run have been studied extensively; for reviews, see Baird et al. (2014); García and Saavedra (2017); Snilstveit et al. (2015).

[2]From this point on, we will refer to both socioeconomic status and self-reported well-being as "well-being" for the sake of brevity.

[3]In developed countries, 21-year-olds may still attend tertiary education, which may complicate long-term analyses of labor market outcomes, for example. In the context we discuss in this paper, no individuals attended tertiary education, and no respondents completed grade 12. In the data we will present, only one respondent completed grade 11, and only one respondent completed grade 10.

program—from a sub-sample of the original study participants. Attrition is relatively high compared to other studies, and it is imbalanced across groups; we therefore implement a number of approaches to account for it.

Our study setup is the following. In 2008, 209 schools in Cambodia were randomly allocated between two treatment arms (104 schools) and a control group (105 schools). In approximately half of the treatment schools (51 schools), students in grade four received a scholarship based on merit—high-performing students were selected using a baseline test of math and language skills. Fourth-graders in the remaining treatment schools received a scholarship based on poverty—students were selected using a poverty index, based on household and family socioeconomic characteristics. Scholarships were given to recipients for three years (i.e., until the completion of primary school), conditional on continued school participation and basic performance standards.

Using the school randomization, we show intention-to-treat (ITT) results on outcomes related to schooling and skills—formal education, cognitive and socio-emotional outcomes—and on outcomes related to labor and well-being. In order to address potential problems of multiple hypothesis testing, we focus our presentation of results on "family indices"—that is, indices that standardize the individual measures in each set of outcomes and calculate a weighted average (following Anderson (2008), by calculating inverse covariance matrix-weighted averages).[4]

We have five main findings. First, the results show a positive impact on the acquisition of formal education (0.189 standard deviations on the education family index, significant at the one-percent level). Despite some catch-up by the control group between 2011 and 2016, scholarship recipients have on average 0.241 more years of schooling than non-recipients. In comparison to their control group peers, treated individuals also improved their primary school completion rate by 8.0 percentage points, and had a 6.8 percentage point higher participation rate in formal education. The magnitudes of these impacts are in line with

[4]We also report results for the individual underlying indicators as Appendix materials.

those of other programs that have attempted to reduce direct costs (for example scholarships; see Kremer et al. (2009), and Duflo et al. (2021)) and indirect costs of education (for example conditional cash transfers; see Fiszbein and Schady (2009)). These effects are slightly higher for poverty-based scholarship recipients than for merit-based recipients (though we cannot rule out that they are equal). Interestingly, the amount of the scholarship was extremely low (US$20 per year), implying a large price elasticity for education.

Second, we find positive effects on cognitive skills for merit-based scholarship recipients. On average, these students score 0.131 standard deviations higher in the cognitive family index than the control group, while we fail to reject equality to zero for the poverty-based students. The effect is especially large for poor students who were offered a merit-based scholarship; the point estimate for the family index is 0.233 standard deviations (significant at the five-percent level). This result suggests that a merit-based scholarship does not necessarily increase inequalities.

Third, we do not find any systematic impacts on indicators related to two sets of measures of socio-emotional outcomes: emotional and behavioral difficulties (as measured by the Strengths and Difficulties Questionnaire, "SDQ") and the "Big 5" personality traits (Openness, Conscientiousness, Extraversion, Agreeableness, and Neuroticism).[5] Our main estimates suggest impacts on these outcomes are systematically close to zero, and not statistically significant.

Fourth, as per the family index, we find no systematic impacts on labor outcomes. We find a small (and marginally significant) effect of the program on the probability of working (2.6 percentage points, from a control mean of 91.9 percent) and a negative (but not statistically significant) effect on yearly earnings. This latter finding is puzzling as we report no negative impacts on labor market participation, the age of labor market entry, or the

[5] We collected information on other socio-emotional outcomes such as grit and growth mindset. However, the psychometric and statistical properties of these measures in our context were weak (Co-Investigator 1 et al. 2022).

cognitive demands of respondents' occupations. We also document a positive, albeit not statistically significant, impact on recipients' daily reservation wage.[6]

Fifth, we find positive impacts on well-being. We report positive point estimates for all six indicators and statistically significant impacts on recipients' perceived socioeconomic status, quality of health, and quality of life. Per the family index, we find that the scholarship program improved recipients' well-being by 0.159 standard deviations (significant at the one-percent level). Merit-based scholarship recipients drive these positive results.

Given the long-run panel nature of our sample, the issue of attrition is an important one. We find that, while there are no systematic differences in the predictors of attrition across groups, and while the samples remain balanced even after attrition occurs, there is nevertheless a significant difference in attrition rates between the treatment and control groups (24.2% in the treatment group versus 32.7% in the control group). Out of concern that this difference may affect our results, we implement a number of approaches to adjust for attrition. A traditional bounding strategy (following Lee (2009)) yields wide bounds and rather uninformative results, whereas our remaining analyses (following Wooldridge (2010) and more advanced strategies developed by Behaghel et al. (2014) and Molina Millán and Macours (2017)) suggest the findings are not driven by attrition.

In addition to these main findings, we explore heterogeneity according to the scholarship recipient's gender.[7] We recognize that our study is weakly powered to detect differential effects; however, we find suggestive evidence that female recipients acquire more schooling than male ones (the effect on the education family index for males is 0.15 standard deviations, while the effect for females is 0.215 standard deviations; both of them statistically significant

[6]It is also important to recognize that our research—just as any of the few other long-term studies—measures effects when individuals are in their early twenties, and as such it is perhaps too early to detect labor effects. We, therefore, interpret the results on labor outcomes with caution since more time might make differences become more apparent—or even reverse the direction of impacts. Moreover, measurement issues (such as noisy data, seasonality, and informality) may especially complicate the ability to detect effects on labor market outcomes (Bouguen et al. 2019).

[7]Effects of the program might differ by gender either because of differential complementarities between skills and the demands in the labor market (Pitt et al. 2012) or because of differential costs and benefits in the decision of investing in education (Becker et al. 2010).

different from zero–but not statistically significant from each other). In contrast, we find positive effects on cognitive outcomes only for males (with a point estimate of 0.18 standard deviations; significant at the 10-percent level) and a negative point estimate of -0.07 standard deviations for females (the difference in effects is statistically significant, at the five-percent level). Similarly, we find that the program's positive effects on well-being outcomes are driven by impacts for males—the effect on the family index is of 0.203 standard deviations for males, and 0.116 standard deviations for females respectively (only the former is statistically significant).[8] Together, these findings may suggest that, in the Cambodian context, females' additional educational investments are neither "rewarded" with increases in cognitive skill, nor with higher returns in the labor market or improved well-being.

All in all, our results point towards significant effects from the scholarship on schooling, but effects on cognitive outcomes only for a certain group—merit-based scholarship recipients and especially for poor individuals among them—and, based on our most conservative estimations, no systematic effects on socio-emotional outcomes. In turn, we do not find clear effects on labor market outcomes, but significant, positive effects on well-being outcomes. These positive effects are driven by merit-based students, potentially indicating that the label attached to the scholarship matters (that is, whether a recipient is portrayed as meritorious, as opposed to calling the recipient poor). Our findings also reveal that male recipients benefited more from the scholarship than female recipients. Our results present a complex picture that nevertheless suggests that demand-side interventions, such as scholarships, and their particular targeting approaches can have important long-term effects.

Our study thus contributes to an emerging literature on the causal, long-term effects of conditional cash transfers (CCTs) and scholarships. While programs vary in design, and contexts matter, the results are quite consistent that CCTs and scholarships, in the long run, have positive effects on school progression (that is, non-treated individuals do not catch up

[8] We find a statistically significant difference in the effects, favoring males, on respondents' subjective social status, their asset ownership, and their perceived quality of health (see Table A21, in the Appendix).

with beneficiaries), and in general, these programs increase formal education. These long-term effects were present in CCT programs in Colombia (Barrera-Osorio et al. 2019), Ecuador (Araujo et al. 2016), Indonesia (Cahyadi et al. 2020), Mexico (Parker and Vogl 2018), and Nicaragua (Barham et al. 2013), as well as in a scholarship program in Ghana (Duflo et al. 2021). The effects on labor outcomes tend to be positive but with some variation depending on the context and recipient gender. Positive effects were found in Nicaragua (ibid.), Kenya (Ozier 2016), and Mexico (ibid.), with heterogeneous effects by gender in Ghana (ibid.), and no effects in Ecuador (ibid.) and Indonesia (ibid.). Three of these long-term studies report delays in fertility and marriage: Ozier (2016) and Brudevold-Newman (2021), both in Kenya, and Duflo et al. (2021) in Ghana. Finally, two studies present causal evidence of positive effects of a scholarship program in Kenya on female empowerment and attitudes (Friedman et al. 2016; Jakiela et al. 2015).[9]

Our contribution to this research is threefold. First, we present impacts of the scholarship program on educational attainment, cognition, and socio-emotional outcomes. We aim to document whether the initial short-term impacts on school progression are sustained. Likewise, we investigate if there is fade out of the initial impacts on cognitive skills that were detected three years after the program started.[10] In addition, ours is one of only very few studies of impacts of schooling on socio-emotional outcomes in a low-income country. Second, we present the long-term effects of a scholarship program on labor market outcomes and well-being. As Molina Millán et al. (2019) point out, other studies of long-term effects are often problematic as control-group students commonly receive the program under investigation eventually. Thus, studies can often only measure differential impacts. In contrast,

[9]Another strand of literature aims to measure the long-term effects of early childhood development interventions (for example, Walker et al. (2007) and Gertler et al. (2014)) and of youth training (Acevedo et al. 2020).

[10]There are several papers in high-income countries suggesting initial fade out from educational interventions (Bailey et al. 2017; Protzko 2015), and long-term effects on outcomes like health and criminal behavior (Anderson et al. 2009; Carneiro and Ginja 2014; Chetty et al. 2011, 2014; Currie and Thomas 2000; Deming 2009; Dynarski et al. 2013; Frisvold and Lumeng 2011; Garces et al. 2002; Heckman et al. 2010; Ludwig and Miller 2007).

in this study, we were able to maintain a comparison with a random group of students who never received a scholarship. Third, our experiment is unique as it allows for the analysis of varying targeting strategies (i.e., merit- versus poverty-based targeting).

Our paper also contributes to the literature that studies how to build socio-emotional (or "non-cognitive") outcomes. While evidence from high-income countries is increasing rapidly, research in low- and middle-income countries remains scant.[11] In these countries, even basic measurement issues are under-researched, with Laajaj and Macours (2021) being among the first to systematically test the cross-context validity of different constructs aimed at capturing socio-emotional skills such as self-esteem, tenacity, conscientiousness, locus-of-control, or adaptability. They find that several of these measures are very noisy and that the cross-country validity of instruments cannot be taken for granted.[12] Beyond these basics, there is little analysis of how socio-emotional skills are built, what role schooling might have in that process, or how the formation of cognitive and socio-emotional skills might interact. The few analyses that have tried to shed light on these relationships are mainly descriptive (Claro et al. 2016; Kyllonen and Bertling 2013).

The remainder of the paper proceeds as follows. In Section 2 we describe in more detail the study setup and context, in Sections 3, 4, and 5 we describe our estimation strategy, the study sample, and data, in Section 6 we present the results, and in Section 7 we provide concluding comments.

[11] See Heckman and Kautz (2014) and West et al. (2016) for an overview, and Deming (2017) for analyses of how the importance of social skills has grown in the U.S. labor market between 1980 and 2012. In high-income countries (and the United States, in particular), several studies have analyzed the production of socio-emotional skills in schools: Blazar (2017); Blazar and Kraft (2017); Jackson et al. (2014); Kraft (2017); Santorella (2018).

[12] A parallel study (Co-Investigator 1 et al. 2022) is analyzing the properties of the various instruments that we use to measure of socio-emotional skills. In this paper, we have retained only those measures that—after extensive field testing and adaptations to the local context—meet a basic threshold of psychometric properties.

2. Intervention and Experimental Design

In 2008, the Government of Cambodia began implementing a new pilot scholarship program for grade four students in 209 public schools.[13] The program's stated goal was to reduce student drop-out rates and increase primary school completion, though the government also implicitly sought to improve students' educational performance. At the time, the program's 209 schools represented all public schools in three of the country's 25 provinces[14] (Mondulkiri, Ratanakiri, and Preah Vihear); the three provinces had been selected for having the highest drop-out rates in the upper primary grades (grades four to six), according to Cambodia's Education Management Information System (EMIS).[15] The program was phased in as a pilot over two years, with a random set of 104 schools starting in 2008/09 and the remaining schools entering in the following year (random assignment was stratified by province).

The scholarship program targeted students entering grade four, using one of two selection approaches. In a randomly selected half of the scholarship schools (51 schools), students were selected based on their combined performance on a test of Khmer and mathematics. This "merit-based" eligibility was determined through a centrally-scored test; the maximum possible score was 25. In the remaining 53 schools, they were selected based on a "poverty-based" approach. A student's "poverty score" was determined based on their self-reported (but validated) household and socioeconomic characteristics; the poverty index ranges from 0 (richest household) to 292 (poorest household).[16] Under both approaches, half of a given school's

[13] The program targeted 210 schools initially; here and elsewhere, we refer to those 209 schools that taught at least one grade-four student, at the beginning of the program.

[14] Here, we count the capital as Cambodia's 25th "province". More precisely, Phnom Penh is a special administrative district whose administrative characteristics partly resemble those of provinces.

[15] To limit the program's geographic scope, in Ratanakiri, only five of seven districts were selected, choosing those districts with the highest dropout rate. In the remaining two provinces, all districts were selected. Dropout rates are correlated with poverty—the study locations represent some of the country's poorest areas.

[16] The aptitude test was based on the 2005/06 Grade Three National Learning Assessment. The poverty assessment asked respondents about household demographics and possession of a list of assets (as provided in Table 2). See Author 1 and Author 3 (2016) for more details on the student assessment and the poverty score.

fourth-graders qualified (i.e., the top half of performers, or the poorest half of students).[17] Crucially (for our study), students in all 209 schools completed both types of assessments, independent of their school's assignment status.

Scholarships were offered to beneficiaries for three years (i.e., through the end of primary school), conditional on their continued enrollment, passing grades, and regular attendance. These requirements were moderately enforced.[18] Scholarships were disbursed as a lump-sum payment of USD20 in the first year, and two payments of USD10 in each of the following two years. As reported by Author 1 and Author 3 (2016), these amounts represent about 3.3 percent of the yearly per capita expenditure in the study sample. These transfers are small compared to similar programs in other countries (Fiszbein and Schady 2009); even relatively small impacts may therefore be cost-effective.

Our experimental design exploits the randomized roll-out of the program over its two phases. In 2008/09, during phase one, fourth-graders in schools that were selected to disburse the program starting in the second phase did not receive any scholarship and did not become eligible in the years thereafter.[19] Note that a subset of these fourth-grade students would have been eligible under one of the two targeting schemes (merit-based or poverty-based), had their school been selected. In expectation, these two sub-samples are equal to their respective eligible peers from phase-one schools (below, we present supporting evidence that the two groups of students are in fact balanced, across phase-one and phase-two schools). Thus, we can identify the causal intention-to-treat (ITT) effect of the scholarship program, under either of the two targeting approaches.[20] Moreover, as phase-one schools were randomly assigned to either the poverty-based or merit-based targeting scheme, we can

[17]Median students also qualified for the scholarship. The number of scholarships was determined using the previous year's official enrolment numbers.

[18]If a student lost her scholarship, its amount could not be reallocated within the same school and the same year. Instead, the amount would be used for the subsequent cohort of fourth-graders.

[19]Recall that the program required students to maintain passing grades. Thus, a phase-one fourth-grader who attended a control group school could not become eligible in phase two by repeating the grade.

[20]By ITT we mean that we are counting a person as treated if they were assigned to be offered a scholarship, whether or not they did what was required to access or retain that scholarship (e.g., enroll, maintain a passing grade).

also compare the scholarship's effect across the two targeting schemes. Figure 1 summarizes this experimental design.

3. Estimation Framework

We derive our approach to estimation and interpreting our results from a conceptual framework that links treatment to outcomes (see Appendix B). In this section we focus on the empirical strategy.

We estimate a generic production function model:

$$Y_{t,i} = \beta_0 + \beta_1 T_{0,i} + \gamma_1 \overline{m}_{0,i} + \gamma_2 \overline{p}_{0,i} + B\boldsymbol{X}_{0,i} + \mu_{t,i} \tag{1}$$

where Y are outcomes such as educational attainment, cognitive skills, socio-emotional skills, labor outcomes, or measures of well-being (which include socioeconomic status, SES). T is an indicator for being offered the scholarship. Consistent with our notation from Figure 1, $\overline{m}$ and $\overline{p}$ are indicators for whether a student would not have qualified for a scholarship under the merit-based (m) and poverty-based (p) targeting scheme, respectively. The inclusion of these control variables allows us to interpret the coefficient on T as the intention-to-treat effect of offering a scholarship. Vector $\boldsymbol{X}_{0,i}$ includes a rich set of baseline characteristics at the student's school, village, and individual level (the next section describes these measures in greater detail). All estimations include province-level fixed effects and allow for the clustering of standard errors at the assignment level (i.e., within schools; cf. Abadie et al. (2023)). Equation 1 estimates an intention-to-treat model, with β_1 capturing the effect of offering a scholarship on outcomes Y, independent of the scholarship's targeting scheme.

We further assess the differential impacts of a merit-based vs. a poverty-based targeting approach in two ways, as shown in Equation 2 and Equation 3.

$$Y_{t,i} = \beta_0 + \beta_1 T_{0,i} + \beta_2 P_{0,i} + \gamma_1 \overline{m}_{0,i} + \gamma_2 \overline{p}_{0,i} + B\boldsymbol{X}_{0,i} + \mu_{t,i} \tag{2}$$

$$Y_{t,i} = \beta_0 + \beta_1 T_{0,i} + \beta_2 P_{0,i} + \beta_3 P\overline{m}_{0,i} + \beta_4 M\overline{p}_{0,i} + \gamma_1 \overline{m}_{0,i} + \gamma_2 \overline{p}_{0,i} + B\boldsymbol{X}_{0,i} + \mu_{t,i} \qquad (3)$$

In Equation 2, we investigate whether effects differ across the two targeting schemes, from a policy perspective. If a social planner seeks to maximize (the aggregate of) treatment effects, then results for Equation 2 will inform her discrete choice for one of the two targeting approaches. Equation 2 adds an indicator P, denoting whether the treatment occurred in a school with the poverty-based targeting scheme. β_1 thus captures the intention-to-treat effect of the scholarship on outcome Y in a merit school, whereas the sum of β_1 and β_2 captures the intention-to-treat effect in a poverty school. Finally, the effect size of (and level of statistical significance for) β_2 indicates whether one targeting strategy dominates the other, from this perspective of maximizing effect sizes.

In turn, in Equation 3, we investigate whether effects differ across the two targeting schemes, *for individuals with comparable baseline characteristics*. Specifically, we compare treatment effects for students who would have qualified under either targeting approach. We would attribute a difference in treatment effects to the presentation of scholarships, or their mere "labeling" as either poverty-based or merit-based. In order to isolate this effect, Equation 3 adds interaction term $P\overline{m}$, denoting students in a school with the poverty-based targeting scheme, who would not have qualified under the merit-based scheme. Equation 3 also adds interaction term $M\overline{p}$, denoting students in a school with the merit-based targeting scheme, who would not have qualified under the poverty-based scheme. β_1 thus captures the intention-to-treat effect of the scholarship on outcome Y for poor, high-scoring students in a merit school, whereas the sum of β_1 and β_2 captures the intention-to-treat effect for poor, high-scoring students in a poverty school. Finally, the effect size of (and level of statistical significance for) β_2 indicates whether one labeling strategy dominates the other, for individuals with comparable baseline characteristics.

4. Sample and Internal Validity

For the present study, we followed a random sub-sample of 1,958 eligible students in 2016.[21]. This sample includes 577 eligible, high-scoring students in schools with the merit-based targeting strategy, 518 eligible, high-poverty students in schools with the poverty-based targeting strategy, and 863 students from control schools who would have qualified under at least one of the two targeting schemes.[22]

Following up a sample after almost 10 years is bound to be difficult, and this proved to be the case in this study. Overall, we have an attrition rate of 27.9 percent with a significant difference of 7.8 percentage points between the treatment and control groups (see Table 1). Column (2) of Table 1 reports the coefficients from bivariate OLS regressions of attrition on baseline characteristics (in a model that includes province fixed effects to account for the study's stratified randomization). Males and worse-off individuals are more likely to attrit. While the overall rate is somewhat higher than in other field studies[23], and there do seem to be some systematic correlates of attrition, we rule out that these correlates are systematically different across treatment and control groups. Column (7) reports the difference-in-difference among attritors and non-attritors, across treatment and control groups (again computed by OLS regression with province fixed effects). Only two out of 16 indicators—indicators of whether the respondent's household has a hard roof and owns pigs—are significant in this difference-in-difference specification. Notably, both reflect asset ownership, yet the coefficient's sign is positive for one and negative for the other. This finding

[21]Our analyses do not include ineligible students who attended the same schools, as they may have been affected by spillover effects. In Figure 1, shaded areas indicate these ineligible students.

[22]Our overall sampling frame consisted of 3,918 eligible fourth-grade students (in the program's 209 schools teaching grade four at baseline), who participated in the baseline assessment, in December 2008 and January 2009. Of those, 1,908 respondents were randomly selected for the first three-year follow-up survey, in 2011. For this first follow-up, an additional 463 eligible "replacement" students were randomly selected, in case students could not be found. In the 2016 follow-up, we tracked all students who had participated in the 2011 study and a random subset of 85 eligible respondents who had not been surveyed thus far (35 attritors from 2011 and 50 newly selected participants, from 2008/2009).

[23]See Ghanem et al. (2021), who show that the median attrition rate in published field studies is around 15 percent——although this includes studies with much shorter follow-up times.

is something we may expect in the case of testing multiple hypotheses. An additional test of the coefficients being jointly equal to zero, using seemingly unrelated estimation (SUR), yields a Chi-square statistic (and corresponding p-value) that does not allow us to reject that the two sub-samples are balanced.[24]

In addition, and despite the different attrition rates between treatment and control groups, the characteristics of the treatment and control populations are balanced at baseline. Table 2 reports means for the treatment and control groups, and then the difference across these groups in a set of observable characteristics at baseline. None of the household characteristics are statistically significantly different across groups (even at the 10-percent level of significance). The only statistically significant difference at baseline between these groups is the indicator of being female: 57.3 percent of students in the treatment groups were girls, whereas 51.7 percent of students in the control group were. Notably, the samples are balanced on the two variables that were used to select scholarship recipients: the poverty index and the baseline test score. To further corroborate this finding (presented in Table 2), we also provide evidence for balance across the two different targeting strategies, in Appendix Table A2.

Based on the findings discussed above, we conclude that the data generally support the fact that our experimental design is valid. First, the correlates of attrition are not different across treatment groups. Nevertheless, to account for the fact that the overall attrition rate differs across treatment and control groups, we perform a number of robustness checks (presented and discussed in Section 6.3). Second, we find that the treatment and control groups are balanced on observables at baseline for our study sample. Finally, to account for the (small) baseline difference in the gender balance across groups, we control for a gender indicator in all regressions and also provide an analysis of heterogeneous effects by gender (in all specifications, we also control for a rich set of baseline variables).

[24] In additional analyses, we investigate the two different targeting strategies, by treating them as separate treatment groups (see Appendix Table A1). SUR suggests that the coefficients may not be jointly equal to zero when using separate samples. Further below we detail our robustness checks, which address this issue.

5. Data and Measurement

Our analysis combines data from five main sources. First, we collect outcome data through in-person interviews at the respondents' residences, using handheld tablets. Second, to construct a variable reflecting intention-to-treat, we use the official government declaration ("Prakas") of scholarship recipients. Third, we match each respondent to baseline data—application forms and baseline tests—as collected in December 2008 and January 2009. Fourth, we construct a vector of control variables through administrative data on baseline school characteristics, as provided by the country's Educational Management Information System (EMIS).[25] Fifth, we take advantage of the fact that Cambodia's 2008 census was conducted just before the scholarship program started. Using geographic coordinates, we match each school to its closest village and include this village's demographic characteristics as additional controls.[26]

Data collection for the baseline and three-year follow-up occurred from December 2008 to January 2009, and from May to September 2011, respectively. Data collection for our latest round of follow-up took from December 2016 to May 2017. We monitored data quality by following standard procedures, as described by Glennerster (2017).[27]

The following discusses our newly collected outcome measures in greater detail. As education outcomes, we measure educational attainment (highest grade completed), whether

[25] We include a binary indicator of whether a school had access to drinking water, a binary indicator of whether the school had a toilet facility, the number of primary school classrooms, the number of newly enrolled fourth-graders, the number of teaching staff, and the school's income.

[26] Village-level data as published by the Cambodian National Institute of Statistics at the Ministry of Planning (2010). We control for the share of villagers who are literate in Khmer, the share of villagers with no schooling, the percentage of villagers engaged in crop or animal farming, the village's population size, and a continuous measure of villagers' household assets.

[27] First, during the first week of fieldwork, we conducted 30 percent of re-surveys ("back-checks", usually within three days) and then reduced this number, for an overall back-check rate of 15.7 percent. Second, we spot-checked approximately 20 percent of interviews, provided immediate feedback, and offered repeat trainings to enumerators. These spot-checks were not only conducted by field supervisors but also through additional, independent field monitoring. Third, we ran daily analytics on newly collected data to spot irregularities, and identify training needs. Finally, we employed 15 percent of staff as dedicated quality-control officers, such that steps to improve data quality could be taken immediately, as part of the regular data flow.

a student completed primary education, and whether the respondent received any formal education since the early three-year follow-up (the latter two are binary variables).

We also collected data on four measures of cognitive skills. First, we administered a computer-adaptive math test, in which respondents answered ten questions from a larger pool of 23 items. We used a three-parameter logistic (3PL) item response theory (IRT) model with a single guessing parameter (Birnbaum 1968; Samejima 1969) to analyze responses to math tests from an evaluation of a similar scholarship program in Cambodia that was targeted to secondary school students (Filmer and Schady 2011, 2014). Participants in this assessment had been tested in two rounds, with overlapping items, and we follow the common (Stocking and Lord 1983) methodology for IRT-based scale equating.[28] Our adaptive test begins with the item of median difficulty. As the test is administered and respondents answer correctly or incorrectly, our assessment picks the next item to be displayed based on maximum information, re-calculates a respondent's ability estimate using expected a posteriori, and continues thereafter until ten items are administered for each respondent (cf. Bock and Mislevy 1982; van der Linden and Pashley 2010). The second assessment is a test of shapes and puzzles loosely based on the Raven's Progressive Matrices. This test is a measure of fluid intelligence; respondents are asked to complete 15 sets of pattern recognition.

Our third measure is a "Digit Span" test, which asks respondents to repeat sequences of single-digit numbers, of increasing length. This test is a common measure of respondents' working memory (Hamoudi and Sheridan 2015). Sequences are presented in sets of two and begin with two integers (asking respondents to repeat 2-1 and 1-3). No additional sequences are asked if a respondent fails to repeat both prompts; the last set of longest sequences presents two strings of eight integers (asking respondents to repeat 6-9-1-7-3-2-5-8 and 3-1-7-9-5-4-8-2).

The fourth outcome is a vocabulary test based on picture recognition, similar to a Peabody Picture Vocabulary Test (PPVT). This test asks respondents to identify the picture

[28] We removed one item with low discrimination.

corresponding to a word that the enumerator reads out loud. For each word, the respondent is asked to select from a choice of four pictures. The test is structured such that items become increasingly difficult (examples of easy items include, "citrus," and "garment"; items of highest difficulty include "vitreous" and "lugubrious"). A maximum of 96 items is presented in sets of 12, and no additional item is displayed if a respondent fails to answer at least five items correctly in a given set. The final skill estimate for each of the math, pattern recognition, and vocabulary recognition tests are calculated with a two-parameter logistic (2PL) IRT model. The Digit Span test score reflects the number of integer sequences a respondent repeated correctly. All four measures are standardized (mean zero and standard deviation of one).

We report on two sets of socio-emotional outcomes: we screen for emotional and behavioral difficulties with the Strengths and Difficulties Questionnaire ("SDQ"), and measure the "Big 5" personality traits. The SDQ represents a common screening instrument; we use (the official Khmer translation of) its most frequently used version with 25 items on psychological attributes (Goodman 1997). Following its scoring guidelines and official recommendations (*ibid.*), we report on three sub-scales, separated into 'internalizing problems' (emotional and peer symptoms, 10 items), 'externalizing problems' (conduct and hyperactivity symptoms, 10 items), and a scale of prosocial behavior (5 items).

To capture respondents' personality traits, the Big Five scale measures five core dimensions of personality. The five broad personality traits measured are Openness, Conscientiousness, Extraversion, Agreeableness, and Neuroticism. Evidence of the Big Five as being relevant (and associated with life outcomes) has been growing, beginning with the research of Fiske (1949) and later expanded upon by other researchers including Norman (1967), Smith (1967), Goldberg (1981), and McCrae and Costa (1987). We use the short 15-item Big Five

Inventory (BFI-S) (Lang et al. 2011), with three items per personality trait. As with the indicators of cognitive skill, all measures of socio-emotional outcomes are standardized.[29]

We also collected information on six labor market outcomes. We ask whether a respondent is currently working (yes or no) and the age at which she or he first started working. We also capture whether the individual participated in any formal or informal training that lasted at least one week, since 2011 (yes or no). We moreover construct a binary indicator of whether a respondent's main work activity is cognitively demanding. We categorize an occupation as such if it requires at least occasional use of reading, writing, mathematics, or a computer (according to the respondent). The survey also asked for respondents' income; our analysis reports on (the inverse hyperbolic sine of) yearly earnings and (the inverse hyperbolic sine of) a respondent's daily reservation wage, i.e., the minimum wage or payment for which a respondent is willing to accept work (both are reported in US dollars).[30]

Our last set of outcomes includes six indicators of socioeconomic status and well-being. We assess subjective social status using a "MacArthur community ladder".[31] Respondents were shown a picture of a ladder with ten rungs and were told that higher rungs correspond to higher socioeconomic status. They were then asked to place themselves on this ladder in relation to everyone in their community. As a second measure of socioeconomic status, we construct an index of respondents' household assets, asking whether they possess items from a list similar to the one presented in Table 2. To calculate an individual's latent SES score, we borrow from the psychometric literature and estimate a two-parameter logistic

[29]For further discussion on these measures, and their psychometric and statistical properties, see Co-Investigator 1 et al. (2022). In addition to the measures we report on here, we collected data on respondents' level of grit (Duckworth and Quinn 2009) and their growth mindset (Dweck 2000). We do not report on results for these measures because of their poor psychometric properties in our data. The inclusion of either of these measures does not substantively change our results.

[30]During the survey, respondents were allowed to answer either in US dollars (a currency commonly used in Cambodia) or in Riel (the local currency). The survey also allowed respondents to provide their answers in terms of varying payment modalities (including in-kind payments, piece-wise pay, and different payment frequencies, for example).

[31]For a description and bibliography of papers that use MacArthur ladders, see the MacArthur Foundation's Network on SES and Health website: http://www.macses.ucsf.edu/Research/Psychosocial/subjective.php.

(2PL) IRT model, placing responses from 2009, 2011, and 2016 on the same scale.[32] We also asked respondents to rate their satisfaction with life at present, all things considered, on a scale from one ("completely dissatisfied") to ten ("completely satisfied") and to rate their quality of life and health, respectively, on a scale from one ("poor") to five ("excellent"). The sixth and last measure screens for (minor) mental health disorders, using the General Health Questionnaire ("GHQ"). We use the short form of the questionnaire (GHQ-12) with Likert scoring (Goldberg and Williams 2006; Quek et al. 2001). All six measures are standardized (mean zero and standard deviation of one).[33]

For each set of educational outcomes, cognitive outcomes, socio-emotional outcomes, labor and SES and subjective well-being, we calculate an overall "family index," following Anderson (2008).[34] These indices have the benefit of reducing the number of statistical tests (and the temptation to selectively focus on positive results). In constructing the indices, we ensured that the qualitative "direction" of the construct was preserved—higher values point to more desirable outcomes. However, our index construction is atheoretical and may group together measurements with different underlying constructs. Therefore, while our main text and conclusions focus on the family indices, we also present results from the individual measurements in Appendix A.

6. Results

We present and discuss our results in two main steps.[35] First, we focus on whether the provision of a scholarship induced higher school accumulation, which in turn may have engendered cognitive and socio-emotional skills. We report impacts on these outcomes of

[32] Filmer and Scott (2012) show that such an IRT approach produces similar household rankings when compared to other aggregation methods.

[33] We standardize by focusing on the endline measures for control group students (who would have qualified for at least one of the two types of scholarships, had they been in a treatment school instead).

[34] We also considered using an alternative index instead, following Kling et al. (2007). The alternative approach does not lead to qualitatively different conclusions.

[35] While one may think of these two as sequential steps, we cannot directly identify causal effects within this sequence. We describe a theoretical model of the relationship between cognitive and socio-emotional outcomes in Appendix B.

offering a scholarship in Table 3. We next turn to the question of whether the different types of scholarships induced different-sized impacts on their target populations. We refer to these as "effects by program type." Because those target populations differ, we then turn to the question of whether the different types of scholarships had different impacts on the set of students who would have qualified for either type (but only received one type, depending on the school they were in). We call this effect the "labeling" effect—the behaviors and attitudes of students, teachers, and family members may differ depending on whether a recipient is portrayed as meritorious, as opposed to calling the recipient poor. Results for both effects by program type and labeling effects are reported in the same table. After these outcomes, we present the impacts of the scholarships on indicators of labor market outcomes and of the socioeconomic status and well-being of individuals (Table 4). In doing so, we also discuss effects by program type and labeling effects for these outcomes. Finally, we investigate whether treatment effects differ by gender (Table 6). All the estimates are intention-to-treat effects.

6.1. Education, Cognitive, and Socioemotional Outcomes

Column (1) of Table 3 reports the program's overall impact on the family index of education outcomes: the increase is of 0.189 standard deviations. Across all indicators, the scholarships had a consistently statistically positive impact on the acquisition of formal education (Table A3). The treatment caused (1) an increase of the highest grade completed of 0.241 grades compared to a control mean of 5.478 grades (a 4.4-percent increase); (2) an increase in the probability of completing primary school by 8.0 percentage points, compared to a control mean of 57.3 percent (a 14-percent increase); and (3) an increase in the probability of receiving any formal education during 2011-2017 of 6.8 percentage points, from a base of 75.4 percent (an increase of over nine percent).[36]

[36] An increase in 0.24 years of schooling is in the range of increases found as a consequence of much larger transfers and expenditures. For example, Behrman et al. (2013) report on a number of evaluations of the demand-side incentive Progresa program in Mexico which was associated with 0.5 additional grades after six years of the program for children who were aged six to eight (pre-program) in rural areas, and of 0.1 to

We now turn to the question of whether we detect any impacts on cognitive outcomes. The estimate for the family index (reported in Column (2) of Table 3) is positive but small (0.05 standard deviations) and not statistically significantly different from zero. Appendix Table 4 reports impacts on the computer-adaptive math test, the progressive matrices assessment, the Forward Digit Span test, and the picture recognition vocabulary test. None of the point estimates is statistically significantly different from zero. From this, we conclude that, overall, the offer of (any) scholarship (and the consequential increase in schooling) had no impacts on cognitive skills development that are detectable in the long term.

Column (3) reports the impact of scholarships on the family index of socio-emotional outcomes. The effect size is a small positive impact of 0.063 standard deviations, but this estimate is not statistically significant (recall that for this index, all outcomes have been re-scaled such that higher values reflect more desirable outcomes). Results for the individual measures confirm this finding (see Appendix Table A5). First, we present results for the Strengths and Difficulties Questionnaire (SDQ)—separating its three factors: Prosocial, internalizing, and externalizing behaviors. Second, we report results for the Big 5—separating its five factors: Openness, Conscientiousness, Extraversion, Agreeableness, and Neuroticism. The only statistically significant impact of the program is on the measure of neuroticism: the program led to a decrease in neuroticism by 0.110 standard deviations, significant at the ten-percent level. We conclude that the offer of a scholarship had no detectable long-term impacts on socio-emotional outcomes.

In sum, these aggregate results indicate systematic effects on school progression and acquisition of formal education but no detectable impacts on long-term cognitive or socio-emotional outcomes. Yet, these results could mask differential impacts by type of scholarship, as documented in the short-term follow-up analysis. The next three columns of Table 3

0.12 additional grades in urban areas for youth aged six to 20 (op. cit. Table 5.1). Progresa transferred to the typical grade 4, 5, and 6 student an average of (approximately) US$15 *per month*, in contrast to the Cambodia transfer of US$20 *per year*. Jackson et al. (2016) show that increasing per-student expenditure by ten percent every year for all twelve years of public schooling in the US would causally lead to 0.31 more years of schooling attained.

report the impacts of the scholarship offer on the respective target population by type of scholarship. The outcome variables are the family indices for education, cognition, and socio-emotional outcomes.[37] The dummy variable structure is such that the coefficient of the overall treatment indicator ("Treatment (any)") captures the ITT effect of the merit-based scholarships on students who met the eligibility criterion for those scholarships; the coefficient on the treatment for poverty schools ("Treatment (poverty)") is the *additional* magnitude of the impact for the poverty-based scholarships on those who met the eligibility criterion for those scholarships (see Equation 2). Note that the statistical significance of this estimate is a test for the difference between the impacts of merit versus poverty scholarships on each of their intended target populations. The sum of these two coefficients is the overall ITT impact of poverty-based scholarships on those who met the eligibility criterion for those scholarships (reported in the bottom panel of the table as "Effect in Poverty Schools").

The impacts on the education family index are positive for both merit- and poverty-based scholarship recipients. Students who were offered a merit-based scholarship scored 0.139 standard deviations higher on the family index, as compared to counterfactual students (Column 4). This impact is 0.106 standard deviation points higher for recipients of poverty-based scholarships, yielding a total impact of 0.245 standard deviations (while both of these point estimates are statistically significant, the difference between them is not, as indicated by the non-significance of the effect from "Treatment (poverty school)").[38]

The impacts on the cognition family index (Column 5) are statistically significant for the merit-based students (a 0.131 standard deviation estimate, significant at the ten-percent level), and negative for the poverty-based treatment (-0.039, not statistically significant). We can rule out that these effects are equal to each other. The results thus indicate that

[37] Our interpretation of Table 3 is that—per their rationale—each family index is a good way to represent the group of impacts in each domain. We, therefore, report the effects using these indices; the results for the individual outcomes that make up the family indices (presented in Appendix Tables A6 to A9) are consistent with those in Table 3.

[38] We speculate that differential impacts by type of scholarship on education acquisition could occur since poverty-based scholarship recipients have lower baseline education levels and that it is easier to increase these from a lower level.

both types of scholarships likely increased the acquisition of schooling, even in the long-term, but only the merit-based induced effects on cognitive outcomes. The short-term differentials across scholarship types identified in the earlier assessment of this program (that is, positive effects on cognitive outcomes for recipients of merit-based scholarships) by Author 1 and Author 3 (2016) were sustained over the long term. In contrast, we do not see a clear pattern of differences in impacts by type of scholarship on the socio-emotional outcomes (Column 6).

These point estimates could potentially mask a different phenomenon: The target populations for the two scholarship types are different. As mentioned above, the poverty-based scholarships had slightly larger point estimates for education than merit-based ones, but this could be because the recipients were somewhat poorer and had lower baseline education indicators.[39] We address this potential effect by isolating the estimated effects to a subset of recipients who would have received a scholarship under either scheme (i.e., they are relatively high-performing students from poor families at baseline), but depending on which school they were attending, they would have (randomly) received a scholarship of one type or another. We call these effects "labeling" effects.[40] Columns (7) to (9) of Table 3 report the results of this model. Here, the structure (per Equation 3) is such that the coefficient of the overall treatment indicator ("Treatment (any)") is the impact of being offered a merit-based scholarship for this sample of high-performing, high-poverty students. The additional impact of being offered a poverty-based scholarship for students with the same profile is captured by the coefficient on the dummy variable for schools in which poverty-based scholarships were distributed ("Treatment (poverty)"). The statistical significance of this coefficient is a test of the difference in impacts for this specific subset of students. The sum of these two

[39]Note that this is not a problem with the experimental design: each of these impacts is derived from comparing a treatment group to a valid counterfactual group—both the treatment and counterfactual groups are different for the estimation of poverty-based and merit-based impacts.

[40]As discussed in Author 1 and Author 3 (2016), differences according to this "labeling" could be driven by a number of factors, for example, motivation associated with being called a "merit recipient," or discouragement associated with being called "poor," or being treated differently by teachers according to these "labels" or by a differential household educational investments across scholarship recipients.

coefficients is the estimate of the impact of poverty-based scholarships and is reported in the bottom panel of the table ("Effect in Poverty Schools, if high scores").[41]

The impact of being offered a merit-based scholarship on the education family index for high-scoring high-poverty students is 0.123 standard deviations. The impact of being offered a poverty-based scholarship for students with the same profile is 0.190 standard deviations. Of these two, only the latter is statistically significant. In addition, the test for the equality of these two effects cannot reject that they are the same. We attribute some of this pattern to the low power we might have—indeed, the additional effect of the poverty-based scholarship is not statistically significantly different from the scholarship-based one, even though the total effects are quite different. In sum, therefore, we take these effects as indicating an overall gain in education, irrespective of the "label" attached to the scholarship.

In contrast, the coefficient for the family index for cognition outcomes is more consistently suggestive of labeling effects. The ITT effect of the offer of a merit-based scholarship on high-scoring, high-poverty students is 0.233 standard deviations (statistically significant, at the five-percent level).[42] The point estimate for a similar student—high-scoring, high-poverty—being offered a poverty-based scholarship is 0.036, which is not statistically significantly different from zero. We can reject that the point estimate is the same as the effect of merit-based scholarships. Labeling a poor student as a "high-achiever" seems to have a positive impact on cognitive outcomes when combined with a scholarship, while the effect of labeling a high-achieving student as "poor" does not.

Finally, none of the results for socio-emotional outcomes suggest much in the way of impacts. All of the effect sizes are small, and none are significantly different from each other. The overall lack of impacts discussed above does not seem to mask any underlying heterogeneity.

[41] These coefficients represent these effects because the model includes two additional dummy variables that capture the impacts of merit-based scholarships on non-poor recipients, and of poverty-based scholarships on low-scoring recipients, which therefore net out the impacts on those sub-populations (see Equation 3).

[42] We note that the impact of these scholarships for high-scoring, low-poverty students is close to zero (and this difference is statistically significant).

6.2. Labor and Well-Being Outcomes

The effects of this program suggest three main findings so far: first, the program induced treated students to increase educational attainment (regardless of the type of scholarship); second, the program caused effects on cognitive outcomes only for merit-based scholarships recipients, especially for high-poverty, high-scoring merit-based students; third, the program did not seem to induce effects on socio-emotional outcomes. The effects on education progression and cognitive outcomes are thus consistent with those identified in the earlier short-run follow-up analysis.

We now turn to our investigation of whether the program had impacts in other dimensions, namely labor market outcomes and measures of socioeconomic status and well-being (Table 4). Overall, the coefficient of impact on the labor family index (Column 1) is small (0.025 standard deviations) and not statistically significant. Appendix Table A10 reports the impacts on the six underlying labor outcomes—whether a respondent is currently working; the age at which the respondent first started working; whether the respondent received any work-related training (since 2011); a measure of the cognitive demands of work; the respondent's average yearly earnings; and the respondent's self-reported daily reservation wage.

The only significant effect of (any) scholarship on labor outcomes is on the probability of currently working, with a point estimate of 2.6 percentage points (in comparison to a control mean of 91.9 percent). All the other point estimates are close to zero, except for a negative effect on yearly earnings, of -0.216 (inverse hyperbolic sine, USD).[43] As before, lack of statistical power may be affecting our ability to statistically detect impacts—an issue that is especially relevant for labor outcomes due to statistical noise, seasonal variability, and informality in the labor market (Bouguen et al. 2019). In summary, we conclude that no clear impact pattern exists on these outcomes.

[43] This results in predicted yearly earnings of USD965 for the control group, and USD708 in the treatment group.

At the same time, the impact on the family index of well-being indicators yields an effect size of 0.159 standard deviations, which is statistically significant (at the one-percent level) (Table 4, Column (2)). As shown in Appendix Table A11, impacts on respondents' subjective social status and the quality of health and life, in particular, are positive and statistically significant (effect sizes are, respectively, 0.176, 0.130, and 0.090 standard deviations). The point estimates of all the other measures point to "improvements"—point estimates for respondents' socioeconomic status index (based on household assets), reported life satisfaction, and quality of life are all positive; the coefficient for the index of health problems is negative. This suggests that while it is hard to identify the channels through which improvements in well-being outcomes have operated, there nevertheless seem to be substantive impacts.

Columns (3) and (4) of Table 4 report impacts on the family indices of labor outcomes and socioeconomic status/well-being by type of scholarship.[44] These results suggest no difference in impacts by type of scholarship on the labor outcomes. In contrast, we detect impacts of merit-based scholarships on the socioeconomic status/well-being, but not for poverty-based scholarships: the effect size for the former is 0.232 standard deviations (significant at the one-percent level), whereas the effect size for poverty-based scholarships is 0.08 standard deviations (and not statistically significant). We can statistically reject that these effects are equal (the point estimate on the variable "Treatment (poverty school)" is -0.152, and statistically significant).

Columns (5) and (6) of Table 4 investigate whether part of these differential effects might come about because of labeling effects. Statistical power can play a role in the noise of these estimates since the cell sizes are becoming quite small (and the outcome measures are noisy).[45] The effect of the merit-based scholarships on high-scoring, high-poverty recipients on the labor family index is 0.126 (not statistically significant), while the effect of poverty-based scholarships on similar recipients is close to zero (0.045, non-significant as well). The

[44]For the results for disaggregated indicators, see Appendix Tables A12 and A13.

[45]Here, we use the term "cell" to refer to either one of the four combinations of students' poverty- and merit-status.

pattern of impacts for socioeconomic status / well-being on this sub-sample (poor and high achievers) is 0.190 for merit-based scholarships (statistically significant at the five-percent level) and close to zero for poverty-based ones. Still, we cannot rule out the equality of coefficients.

In sum, we detect positive impacts on measures of well-being, and these impacts appear to be driven by students receiving merit-based scholarships. The results on the labeling of the scholarships are consistent with impacts of merit scholarships and no impacts of poverty scholarships on poor and high-achieving students.

6.3. Attrition

As discussed in Section 4, our sample exhibits differential attrition rates across the treatment and control groups. We noted that the correlates of attrition are not systematically different across treatment and control groups, and both the baseline and the non-attriting samples are balanced on observables. Concerned that attrition might nevertheless affect our results, we now explore the extent to which our findings are robust to different approaches to accounting for it.

We present three types of robustness checks. First, we calculate Lee (2009) bounds. In this approach, lower and upper bounds for effect sizes are inferred by re-estimating the model after trimming the data from the treatment group (since it suffers less from attrition) either from below or above (based on the outcome variable) such that the resulting samples reflect the same shares of the baseline treatment and control samples.

Applying Lee (2009) often produces wide bounds; therefore, in a second step, we implement the approach put forth in Behaghel et al. (2014). In this approach, additional information that predicts a study participant's reluctance to respond to the survey—e.g., the number of attempts to reach a participant—is introduced in order to rank individuals according to their latent reluctance. The sample is then restricted to respondents with the same distribution of the latent value. When the predicting variable cannot equalize the response rates in the two groups (e.g., because it is overly discrete), Lee bounds (based on trimming excess respondents) are used in addition to the latent distribution equalization. While Behaghel et al. (2014) predict reluctance to respond with the number of attempts needed to obtain a response, we implement three alternative approaches. We use: (a) the number of days required to track a respondent in 2017; (b) a respondent's predicted probability of attriting using tracking and attrition information from the previous (2011) follow-up only; (c) a combination of both of these.

In a third approach to accounting for attrition, we apply inverse-probability weights (IPW) to derive estimates following Wooldridge (2010) and Molina Millán and Macours (2017). We implement three alternative approaches to modeling the probability of attrition, starting with a basic model that uses only baseline correlates of attrition (Column 2 of Table 1). We then follow Molina Millán and Macours (2017) and augment this model by exploiting similarities between difficult-to-find respondents and attritors. This leads us to augment the basic model in two ways: first, including information from the previous (2011) follow-up; second, further adding in information from our 2017 tracking efforts.

We report the results from this set of analyses in three tables. Table 5 reports results corresponding to those for overall average treatment effects. The results in Appendix Table A14 correspond to those for the results that allow for a difference in impacts across the two types of scholarships. The results in Appendix Table A15 correspond to those focusing on the labeling effect.

The first finding from this analysis is that Lee (2009) bounds are very wide. For several of the outcomes, the bounds range from negative and significant to positive and significant, suggesting that this approach to adjusting for attrition is not informative in our setting. We note that this phenomenon has been found elsewhere (for example Kremer et al. (2009)), and is cited as the motivation for Behaghel et al. (2014).

Our main estimates of the overall average treatment effects are very robust to accounting for attrition using the approaches other than Lee bounds. For example, the pattern of average positive and significant impacts on education and SES/well-being but insignificant impacts on cognition, socio-emotional, and labor outcomes is replicated in five out of the seven of these approaches (Table 5). In the two remaining approaches (IPW models that include tracking and attrition in the previous round), the results suggest that impacts on socio-emotional skills and labor outcomes may be positive and significant. Because this finding is not robust, we take the conservative approach and do not attempt to infer that the program had positive and significant impacts on these outcomes.

Similar to the overall average treatment effects, those that allow for a difference in impacts across scholarship types are robust to adjusting for attrition.[46] The finding that the impacts of poverty scholarships are larger than those of merit scholarships on the education index—but both are statistically significant—is replicated in five of the seven approaches used (Column 1 of Appendix Table A14). Under the two other approaches (IPW that include information from the previous round), the impact of the merit scholarships becomes small and insignificant. The result that only merit scholarships impact cognition is similarly robust to accounting for attrition (Column 2 of Appendix Table A14). In this case, however, while the size of the coefficient is stable across approaches, the effect is not statistically significant in some of them. The findings on the other outcomes (small and insignificant impacts of both scholarship types on socio-emotional and labor outcomes, positive and significant impacts of merit scholarships only on SES/well-being) are stable across the various approaches.[47]

Last, we confirm that the labeling effects we find in our main results are also robust to the different approaches to accounting for attrition. The finding that education impacts are largely driven by poverty scholarships is confirmed in all approaches (although in three of the seven approaches, the statistical significance of that effect is lost despite the effect size being similar; Column 1 of Appendix Table A15). The finding that among recipients with

[46] Since Lee (2009) bounds are not informative for the average effects, we do not report them for the effects by program type or labeling effects. The estimates are available from the authors upon request.

[47] In the case of socio-emotional and labor outcomes, some impacts become larger and statistically significant in the IPW approaches that include previous round information. As before, we take the conservative approach and do not place much weight on these specific estimates.

similar backgrounds only those receiving the scholarship labeled as merit see improvements in cognition is confirmed in all of the approaches (with statistical significance lost, but similar effect sizes, in two of those). The pattern of results for labeling effects for the socio-emotional and labor outcomes, as well as those for SES/well-being, likewise follow those for effects by program type—and confirm the main results.[48]

Taken together, we take these results as confirming that our main estimates, and our interpretation of them, are not driven by differential attrition across treatment and control groups. Overall the pattern of results is robust. While some individual estimates in some of the adjusted models lose significance, the point estimates are rarely affected. In some rare cases, impact estimates become statistically significant, but since these are isolated and not robust to the adjustment used, we take the conservative approach of not placing much weight on them.

6.4. Differential Effects by Gender

We now turn to another potential source of heterogeneity in program impacts, namely the gender of the scholarship recipient. There are a number of reasons why one might expect impacts to differ. For example, differences in early-child investments might mean that they have different skills at the time of the program, which would lead to differential impacts on education outcomes, or there may be different complementarities between skills and the demands in the labor market (Pitt et al. 2012), which could lead to different investments and outcomes. More generally, there may be gender-differentiated costs and benefits in the decision to invest in education (Becker et al. 2010).[49]

Table 6 reports impacts differentiated by gender. We estimate each model of the outcome of interest (five family indices) as a function of a dummy variable for having been offered

[48]As yet another robustness check, in Appendix Table A16, we replicate the main findings on impacts after three years, as reported by Author 1 and Author 3 (2016), with the sample of students who did not attrit after nine years. There are no substantial changes to those results.

[49]For these analyses, it is important to keep in mind the slight imbalance across treatment and control groups in the gender ratio (see Table 2).

a scholarship ("Treatment"), an indicator for being female ("Female"), and the interaction between these two.[50] The structure is such that the coefficient of the overall treatment variable is the effect on males, and the coefficient on the interaction term is the additional impact on females. The sum of these two coefficients is the overall impact on females and is reported in the bottom part of the table ("Effect for females").

We find positive impacts on the education progression of females, with an effect size of 0.215 standard deviations (statistically significant). The point estimate for males is 0.152 (statistically significant as well), but we cannot rule out that the two estimates are equal. In contrast, we find differential impacts on cognition. The point estimate for males is 0.180, while the coefficient of the female-treatment interaction is -0.250 standard deviations lower (significant at the five-percent level). The total effect for females is -0.07 standard deviations (indistinguishable from zero). In addition, the impact on the family index for socioeconomic status/well-being is positive for males, with an effect size of 0.203 standard deviations, and the effect for females is 0.116 (non-statistically significant). We cannot statistically rule out that these are equal, but we nevertheless find suggestive evidence that the positive impact for males dominates the effect for females.[51] The point estimates for the other outcomes (family indices for socio-emotional and labor outcomes) for females are all close to zero and not statistically significantly different from zero. It is striking that all the point estimates for males ("Treatment") are positive, while the point estimates of the interaction female-treatment are negative (with the exception of the education family index). Together, these findings suggest that, in this Cambodian context, educational investments are "rewarded" less strongly for females than for males. In Appendix C, we provide additional information on gender-based differences in the returns to education in the Cambodian context.

[50] For the corresponding results for disaggregated indicators, see Appendix Tables A17 to A21. Appendix Table A22 offers results by gender and program type.

[51] As reported in Table A21 (in the Appendix), we find a statistically significant difference in the effects on respondents' subjective social status, their asset ownership, and their perceived quality of health, favoring males.

7. Conclusion

This study has investigated the long-term impacts of a scholarship program that increased schooling, with a particular focus on links between schooling, the development of cognitive and socio-emotional skills, and labor market and well-being outcomes later in life. To this end, we evaluated the effects of a primary school scholarship program in rural Cambodia, nine years after the program's inception, tracking study participants when they were, on average, 21 years old.

Our results point to systematic impacts on school progression and acquisition of formal education but no average impacts on long-term cognitive or socio-emotional outcomes. We do identify positive impacts on cognitive skills among merit-based scholarship recipients, especially among poor (and high-achieving) students. While we find no positive impacts on average on labor market outcomes, we find that various measures of well-being have improved among recipients of merit-based scholarships. There is suggestive evidence of systematic gender differences in these long-term impacts. Our robustness checks moreover suggest that positive impacts on socio-emotional skills and on labor outcomes may be masked by systematic attrition.

We note two factors that are important to keep in mind when interpreting these results. First, they reflect the effect of increasing schooling by only about four additional months. While these may be critical months, inasmuch as the program induced individuals to finish primary education (the effect is large: the scholarship induced an 8 percentage point increase in primary completion), it is possible that some of the key impacts of schooling on socio-emotional skills happen early on (when both the control and treatment groups were still in school) or later in adolescence (when, for this population, both groups might have left school). Second, our relatively limited sample size may have reduced the precision of some estimates.

Our study has several implications. Prior research has argued that more schooling does not necessarily imply more learning (The World Bank 2018); in turn, our work highlights that more schooling—even if it enhances learning in the short run—may not lead to measurable improvements in socio-emotional skills, and not necessarily to noticeable improvements in labor market outcomes among young adults. We note that it is possible that some of the positive effects might only manifest themselves later, as has been argued in the context of some long-run studies of early child development interventions (see discussion in Duncan and Magnuson (2013) for how this might explain findings from the United States). Indeed, our finding of positive impacts on long-term well-being suggests that there may be such unmeasured channels at work here. Importantly, our research also shows that targeting matters for cognitive outcomes. Nevertheless, additional research is needed in at least two main areas to better understand this puzzle.

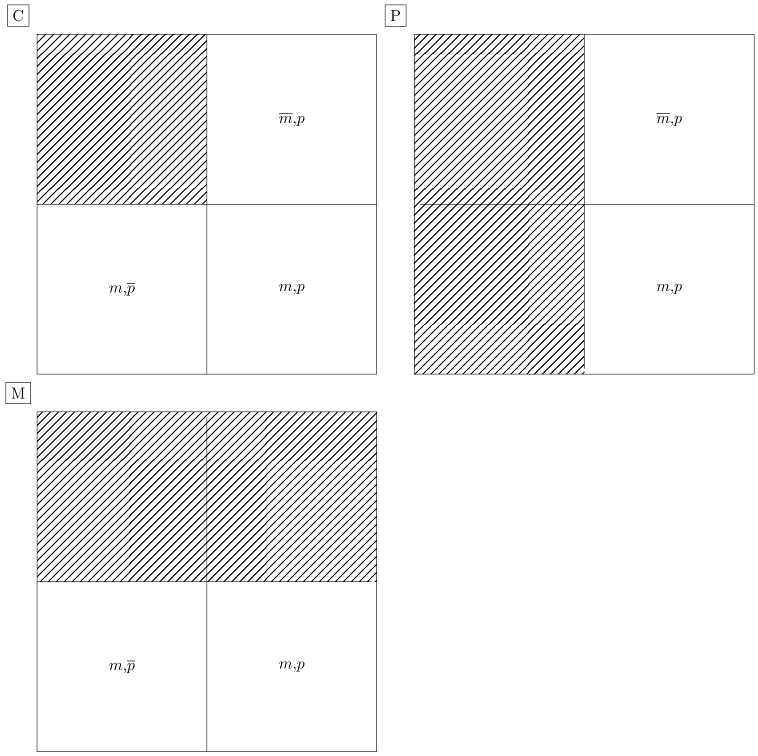
First, our findings are consistent with research by Jackson (2018), suggesting that the school-based production of cognitive skills may not *necessarily* go hand-in-hand with improvements in socio-emotional outcomes. However, research on how to purposefully foster socio-emotional skills in school settings is only in its infancy, especially in low- and middle-income countries (see West et al. (2016) for an example from the United States). Moreover, much more research is needed to develop and validate robust measures of socio-emotional skills that are appropriate for different cultural contexts (see Laajaj and Macours (2021) for an analysis and discussion of this challenge).

Second, our analysis of heterogeneous effects provides suggestive evidence that labor market effects may be concentrated among poorer beneficiaries and among beneficiaries who are male. This result echoes the findings of Duflo et al. (2021), who find labor market effects for a subset of male students only. It will be important to understand how programs such as these can be designed in a way such that they also fully benefit female recipients.

Finally, we conclude with an implication for policy. The fact that it was poor recipients who received a merit-based scholarship that eventually had higher cognitive skills and that it was merit-based scholarship recipients that ultimately reported better measures of well-being suggests that there is not necessarily a trade-off between equity and efficiency when choosing how to target such a program. As shown by Haushofer et al. (2022), policymakers may thus improve over purely poverty-based targeting approaches even if their preferences are highly redistributive. Of course, this may be very context-specific (depending, for example, on a relatively low correlation between socioeconomic status and test scores at baseline), but it nevertheless suggests that there may be flexibility in how to design targeting schemes that reach both equity and efficiency objectives.

Figures and Tables

Figure 1: Experimental Design



Notes. This figure depicts the study's experimental design. Schools were randomly assigned to either a condition with merit-based scholarships (M), to a condition with poverty-based scholarships (P), or to a control condition (C). m and p denote whether students qualify under the merit- or poverty-based targeting schemes, respectively; $\overline{m}$ and $\overline{p}$ denote the opposite. As discussed in Sections 3 and 4, our analyses focus on eligible students in treatment schools and their peers in control schools; shaded areas indicate students we drop from our analyses.

Table 1: Analysis of Attrition

		OLS	Descriptives				Diff-in-Diffs
	n	Coeff.	Non-attritor T	Attritor T	Non-attritor C	Attritor C	Coeff.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Female	1915	-0.099***	0.573	0.419	0.517	0.422	-0.053
		(0.022)	(0.495)	(0.494)	(0.500)	(0.495)	(0.055)
Number of minors	1879	0.007	1.74	1.672	1.713	1.849	-0.201
		(0.011)	(1.075)	(1.129)	(1.109)	(1.110)	(0.127)
Own motorcycle	1894	-0.027	0.328	0.29	0.376	0.363	-0.038
		(0.023)	(0.470)	(0.454)	(0.485)	(0.482)	(0.049)
Own car/truck	1877	-0.042	0.11	0.055	0.1	0.099	-0.043
		(0.041)	(0.313)	(0.229)	(0.300)	(0.299)	(0.033)
Own oxen/buffalo	1900	-0.077***	0.541	0.404	0.498	0.42	-0.035
		(0.027)	(0.499)	(0.492)	(0.500)	(0.494)	(0.059)
Own pig	1906	-0.048*	0.577	0.432	0.533	0.535	-0.132**
		(0.025)	(0.494)	(0.496)	(0.499)	(0.500)	(0.057)
Own ox or buffalo cart	1878	-0.083***	0.279	0.183	0.263	0.181	0.004
		(0.027)	(0.449)	(0.387)	(0.441)	(0.386)	(0.045)
Hard roof	1880	-0.087***	0.47	0.41	0.475	0.32	0.094*
		(0.023)	(0.499)	(0.493)	(0.500)	(0.467)	(0.056)
Hard wall	1908	-0.021	0.493	0.473	0.516	0.468	0.034
		(0.024)	(0.500)	(0.500)	(0.500)	(0.500)	(0.057)
Hard floor	1897	0.006	0.87	0.835	0.823	0.845	-0.048
		(0.034)	(0.336)	(0.372)	(0.382)	(0.363)	(0.041)
Have automatic toilet	1889	0.086	0.029	0.054	0.041	0.044	0.023
		(0.064)	(0.168)	(0.227)	(0.198)	(0.205)	(0.023)
Have pit toilet	1889	-0.01	0.124	0.151	0.117	0.091	0.048
		(0.039)	(0.329)	(0.358)	(0.322)	(0.288)	(0.040)
Electricity	1905	0.003	0.188	0.205	0.226	0.212	0.027
		(0.030)	(0.391)	(0.405)	(0.419)	(0.410)	(0.048)
Piped water	1901	0.022	0.029	0.046	0.042	0.033	0.025
		(0.068)	(0.167)	(0.209)	(0.201)	(0.179)	(0.023)
Poverty Index (0-292)	1958	0.000**	223.165	233.74	225.697	230.652	3.915
		(0.000)	(56.281)	(46.274)	(49.008)	(46.840)	(6.061)
Test score (0-25)	1958	-0.004	19.248	18.966	18.33	18.248	-0.299
		(0.003)	(3.929)	(4.300)	(4.348)	(4.432)	(0.536)
Joint significance: Ho: all coef. =0							
Chi-square							22.62
p-value							0.12
Attrition rate	1958			0.242		0.327	-0.078***
							(0.029)

Notes. All variables measured at baseline. Column (2) displays coefficients computed by bivariate OLS regressions of an attrition indicator on covariates, controlling for province fixed effects. Columns (3) to (6) display the means for the control group attritors, the treatment group attritors, the control group surveyed and the treatment group surveyed. Standard deviations in parentheses. Column (7) is the difference between the treatment group mean and the control group mean among attritors minus the difference between the treatment group mean and the control group mean among respondents. Differences in means are computed by OLS regression, controlling for province fixed effects. All standard errors in parentheses are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1. The Chi-square (and corresponding p-value below) is the result of a test testing for the individual coefficients being jointly equal to 0 using seemingly unrelated estimation.

Table 2: Balance at Baseline

	n (1)	All (2)	Treatment (3)	Control (4)	Difference (5)
Female	1378	0.55	0.573	0.517	0.049*
		(0.498)	(0.495)	(0.5)	(0.029)
Number of minors	1349	1.729	1.74	1.713	-0.028
		(1.089)	(1.075)	(1.109)	(0.101)
Own motorcycle	1362	0.348	0.328	0.376	-0.005
		(0.477)	(0.47)	(0.485)	(0.038)
Own car/truck	1351	0.106	0.11	0.1	0.025
		(0.308)	(0.313)	(0.3)	(0.025)
Own oxen/buffalo	1366	0.523	0.541	0.498	0.032
		(0.5)	(0.499)	(0.5)	(0.041)
Own pig	1372	0.559	0.577	0.533	0.038
		(0.497)	(0.494)	(0.499)	(0.041)
Own ox or buffalo cart	1351	0.272	0.279	0.263	0.007
		(0.445)	(0.449)	(0.441)	(0.036)
Hard roof	1349	0.472	0.47	0.475	0.021
		(0.499)	(0.499)	(0.5)	(0.041)
Hard wall	1372	0.502	0.493	0.516	0.004
		(0.5)	(0.5)	(0.5)	(0.044)
Hard floor	1366	0.851	0.87	0.823	0.035
		(0.357)	(0.336)	(0.382)	(0.033)
Have automatic toilet	1356	0.034	0.029	0.041	-0.006
		(0.181)	(0.168)	(0.198)	(0.015)
Have pit toilet	1356	0.121	0.124	0.117	0.016
		(0.326)	(0.329)	(0.322)	(0.03)
Electricity	1374	0.204	0.188	0.226	-0.012
		(0.403)	(0.391)	(0.419)	(0.035)
Piped water	1367	0.034	0.029	0.042	-0.005
		(0.182)	(0.167)	(0.201)	(0.015)
Poverty Index (0-292)	1411	224.208	223.165	225.697	-5.68
		(53.403)	(56.281)	(49.008)	(4.722)
Test score (0-25)	1411	18.87	19.248	18.33	0.383
		(4.13)	(3.929)	(4.348)	(0.46)
Joint significance: Ho: all coef. =0					
Chi-square					7.98
p-value					0.95

Notes. Minors refers to respondents age 14 and under; this may include the respondent. HH size refers to the number of people living in the respondent's household, including the respondent. Married is a dummy equal to 1 if the respondent is currently married and 0 if never married, divorced or separated. This variable is missing for minors. Currently working is a dummy equal to 1 if the respondent worked during the last week or has a job at the moment and 0 otherwise; respondents may work and also be a student. Column (1) presents the number of observations with endline information. Colums (2) to (4) display the means for all observations, the treatment group, and the control group, respectively. Standard deviations in parentheses. Column (5) is the difference between the treatment group mean and the control group mean. Differences in means are computed by OLS regression, controlling for province fixed effects. All standard errors in parentheses are clustered at the school level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The Chi-square (and corresponding p-value below) is the result of a test testing for the individual coefficients being jointly equal to 0, using seemingly unrelated estimation.

Table 3: Effects on Schooling and Skills

	Pooled Effects			Effects by Program Type			Labeling Effects		
	Family index: Education (1)	Family index: Cognition (2)	Family index: Socioemotional (3)	Family index: Education (4)	Family index: Cognition (5)	Family index: Socioemotional (6)	Family index: Education (7)	Family index: Cognition (8)	Family index: Socioemotional (9)
Treatment (any)	0.189***	0.005	0.063	0.139*	0.131*	0.074	0.123	0.233**	0.126
	(0.067)	(0.065)	(0.064)	(0.077)	(0.078)	(0.078)	(0.105)	(0.104)	(0.113)
Treatment (poverty school)				0.106	-0.169*	-0.023	0.067	-0.197*	0.002
				(0.094)	(0.093)	(0.093)	(0.107)	(0.105)	(0.111)
Treatment (merit school and non-poor)							-0.008	-0.201	-0.067
							(0.126)	(0.141)	(0.151)
Treatment (poverty school and low scores)							0.162	-0.125	-0.195
							(0.110)	(0.141)	(0.158)
Observations	1,370	1,369	1,360	1,370	1,369	1,360	1,370	1,369	1,360
R-squared	0.130	0.150	0.042	0.132	0.153	0.042	0.133	0.155	0.043
Effect in poverty schools				0.245***	-0.039	0.051			
				(0.086)	(0.082)	(0.081)			
Effect in poverty schools, if high scores							0.190*	0.036	0.129
							(0.103)	(0.103)	(0.110)
F-statistic	4.058	8.617	2.984	4.283	8.402	2.920	4.338	8.128	2.920
Control mean	0.039	0.018	-0.003	0.039	0.018	-0.003	0.060	0.033	-0.082

Notes. Estimated treatment effects. The dependent variables are family indices. They represent the inverse covariance matrix-weighted mean of standardized variables following Anderson (2008). For education, the index consists of (1) the highest grade the individual completed (2), a dummy equal to 1 if the individual completed primary education, and (3) a dummy equal to 1 if the individual was enrolled in the formal education system during any of the years 2011 to 2016. For cognitive skills, it consists of (1) the score on the mathematics computer adaptive test, computed using Item Response Theory (IRT) with a two-parameter logistic (2PL) model, (2) the score on the Raven's matrices test computed using IRT with a 2PL model, (3) the score on the digit span test using forward items only, and (4) the score on a Picture Recognition Vocabulary Test computed using IRT with a 2PL model. In the panel in the middle (effects by program type), treatment (any) captures effects for students who received the treatment under the merit-based scheme. For socioemotional skills, it consists of (1) the score on the pro-social facet (the higher the score, the more pro-social) of the Strengths and Difficulties Questionnaire (SDQ), (2) the score on the internalizing behavior facet (the higher the score, the more externalizing behavior problems) of the SDQ, standardized, (3) the score on the externalizing behavior facet (the higher the score, the more externalizing behavior problems) of the SDQ, and (4) the scores on the Openness, Conscientiousness, Extraversion, Agreeableness, and Neuroticism facets of the Big Five scale. Each score is "flipped" so higher values indicate a more desirable outcome. Effect in Poverty Schools captures effects for students who received the treatment under the poverty-based scheme. In the panel to the right (labeling effects), treatment (any) captures effects for students who received the treatment under the merit-based scheme but would have also qualified under the poverty-based scheme. Effect among high performers in Poverty Schools captures effects for students who received the treatment under the poverty-based scheme but would have also qualified under the merit-based scheme. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables, and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Effects on Labor and Well-being Outcomes

	Pooled Effects		Effects by Program Type		Labeling Effects	
	Family index: Labor outcomes	Family index: SES/Well-being	Family index: Labor outcomes	Family index: SES/Well-being	Family index: Labor outcomes	Family index: SES/Well-being
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment (any)	0.025	0.159***	0.031	0.232***	0.126	0.190**
	(0.053)	(0.055)	(0.062)	(0.070)	(0.090)	(0.090)
Treatment (poverty school)			-0.013	-0.152*	-0.080	-0.149
			(0.078)	(0.087)	(0.093)	(0.097)
Treatment (merit school and non-poor)					-0.221*	0.075
					(0.129)	(0.120)
Treatment (poverty school and low scores)					0.018	0.080
					(0.145)	(0.158)
Observations	1,241	1,354	1,241	1,354	1,241	1,354
R-squared	0.104	0.109	0.104	0.112	0.107	0.112
Effect in poverty schools			0.017	0.080		
			(0.069)	(0.071)		
Effect in poverty schools, if high scores					0.045	0.041
					(0.090)	(0.087)
F-statistic	4.210	6.207	4.106	6.104	4.080	5.949
Control mean	-0.001	-0.002	-0.001	-0.002	-0.061	-0.088

Notes. Estimated treatment effects. The dependent variables are family indices. They represent the inverse covariance matrix-weighted mean of standardized variables following Anderson (2008). For labor market outcomes, the index consists of (1) a dummy equal to 1 if the individual is currently working, (2) the age at which the individual started to work, (3) a dummy equal to 1 if the individual participated in any formal or informal training that lasted at least one week, since 2011, (4) a dummy equal to 1 if the main work activity demands cognitive ability (read, write, calculate, or use a computer), (5) the yearly earnings expressed in US dollars and transformed using an inverse hyperbolic sine, and (6) the daily reservation wage in US dollars and transformed using an inverse hyperbolic sine. Treatment captures effects for students who received any treatment under either scheme. In the panel in the middle (effects by program type), treatment captures effects for students who received the treatment under the merit-based scheme. Effect in Poverty Schools captures effects for students who received the treatment under the poverty-based scheme. In the panel to the right (labeling effects), treatment (any) captures effects for students who received the treatment under the merit-based scheme but would have also qualified under the poverty-based scheme. Effect among high performers in Poverty Schools captures effects for students who received the treatment under the poverty-based scheme but would have also qualified under the merit-based scheme. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Sensitivity to Differential Attrition

	Family index: Education (1)	Family index: Cognition (2)	Family index: Socioemotional (3)	Family index: Labor outcomes (4)	Family index: SES/Well-being (5)
Lee (2009) bounds					
Lower bound					
Treatment (any)	0.090	-0.139**	-0.168***	-0.168***	-0.039
	(0.069)	(0.062)	(0.060)	(0.051)	(0.051)
Upper bound					
Treatment (any)	0.429***	0.241***	0.278***	0.229***	0.365***
	(0.058)	(0.061)	(0.060)	(0.050)	(0.052)
Behaghel et al. (2014): Number of days tracking in 2017					
Lower bound					
Treatment (any)	0.171**	0.014	0.067	0.020	0.179***
	(0.069)	(0.066)	(0.065)	(0.054)	(0.058)
Upper bound					
Treatment (any)	0.172**	0.018	0.071	0.021	0.183***
	(0.069)	(0.066)	(0.065)	(0.054)	(0.057)
Behaghel et al. (2014): Probability of attrition based on 2011 tracking and attrition					
Treatment (any)	0.228***	0.065	0.055	0.023	0.158***
	(0.069)	(0.067)	(0.069)	(0.055)	(0.058)
Behaghel et al. (2014): Probability of attrition based on 2011 tracking and attrition, 2017 tracking					
Treatment (any)	0.191***	0.052	0.056	0.023	0.164***
	(0.068)	(0.065)	(0.064)	(0.053)	(0.056)
IPW: Baseline covariates					
Treatment (any)	0.186***	0.053	0.067	0.031	0.154***
	(0.067)	(0.066)	(0.064)	(0.054)	(0.055)
IPW: 2011 tracking and attrition					
Treatment (any)	0.178**	-0.002	0.133*	0.169**	0.177***
	(0.079)	(0.081)	(0.079)	(0.075)	(0.064)
IPW: Baseline covariates, 2011 tracking and attrition, 2017 tracking					
Treatment (any)	0.175*	0.071	0.218**	0.200**	0.122*
	(0.099)	(0.096)	(0.095)	(0.100)	(0.074)

Notes. Estimated treatment effects. The dependent variables in columns (1) to (5) are the family indices from Tables 3 and 4. Treatment captures effects for students who received any treatment under either scheme. Lee (2009) and Behaghel et al. (2014) estimates tightened within three eligibility categories measured at baseline (high-performing and less poor, low-performing and poor, and high-performing and poor). Probability of attrition estimated through a probit regression of an attrition indicator on treatment status and covariates. "Baseline covariates" as listed in Table 1. "2011 tracking" information includes a 2011 indicator of attrition, the number of days needed for tracking, and its quadratic. "2017 tracking" refers to the study's five stages of respondent tracking, following Molina Millán and Macours (2017). All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table 6: Heterogeneous Treatment Effects by Gender

	Family index: Education (1)	Family index: Cognition (2)	Family index: Socioemotional (3)	Family index: Labor outcomes (4)	Family index: SES/Well-being (5)
Treatment	0.152**	0.180*	0.115	0.080	0.203***
	(0.075)	(0.093)	(0.084)	(0.075)	(0.072)
Female and treatment	0.063	-0.250**	-0.088	-0.123	-0.086
	(0.102)	(0.117)	(0.111)	(0.099)	(0.109)
Female	-0.093	-0.196*	-0.018	-0.258***	-0.151
	(0.102)	(0.114)	(0.102)	(0.089)	(0.099)
Observations	1,337	1,336	1,328	1,209	1,321
R-squared	0.132	0.154	0.046	0.106	0.109
Effect for females	0.215**	-0.070	0.027	-0.043	0.116
	(0.091)	(0.084)	(0.085)	0.069	(0.082)
F-statistic	4.302	9.145	3.246	4.147	5.971
Covariates	Yes	Yes	Yes	Yes	Yes
Control mean (females)	-0.015	-0.086	-0.041	-0.159	-0.140

Notes. Estimated treatment effects. The dependent variables in columns (1) to (5) are the family indices from Tables 3 and 4. Treatment captures effects for male students who received any treatment, under either scheme. Effect for females captures the respective effect for female students. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

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Online Appendix

Long-Term Impacts of Merit- and Poverty-Based Primary School Scholarships: Evidence from Cambodia

Appendix A Additional Tables

Table A1: Analysis of Attrition, by Targeting Approach

	Merit Scholarship					Poverty Scholarship				
	Attritor C (1)	Attritor T (2)	Non-attritor C (3)	Non-attritor T (4)	Diff-in-Diffs (5)	Attritor C (6)	Attritor T (7)	Non-attritor C (8)	Non-attritor T (9)	Diff-in-Diffs (10)
Female	0.452	0.391	0.487	0.491	-0.062	0.485	0.402	0.518	0.632	-.184**
	(.499)	(.49)	(.501)	(.501)	(.072)	(.501)	(.492)	(.5)	(.483)	(.082)
Number of minors	1.831	1.527	1.739	1.631	-0.184	1.899	1.83	1.81	1.864	-0.149
	(1.132)	(1.09)	(1.124)	(1.091)	(.165)	(1.111)	(1.154)	(1.117)	(1.038)	(.188)
Own motorcycle	0.373	0.355	0.44	0.424	-0.001	0.266	0.232	0.274	0.227	-0.014
	(.485)	(.481)	(.497)	(.495)	(.075)	(.443)	(.424)	(.447)	(.419)	(.063)
Own car/truck	0.114	0.082	0.117	0.174	-0.08	0.024	0.027	0.024	0.042	-0.008
	(.319)	(.275)	(.322)	(.38)	(.05)	(.152)	(.162)	(.153)	(.202)	(.026)
Own oxen/buffalo	0.458	0.427	0.543	0.587	-0.058	0.361	0.384	0.381	0.487	-0.048
	(.5)	(.497)	(.499)	(.493)	(.079)	(.482)	(.489)	(.486)	(.501)	(.074)
Own pig	0.554	0.427	0.545	0.619	-.198***	0.479	0.446	0.432	0.535	-0.11
	(.499)	(.497)	(.499)	(.486)	(.072)	(.501)	(.499)	(.496)	(.499)	(.078)
Own ox or buffalo cart	0.181	0.182	0.284	0.302	0	0.16	0.188	0.179	0.227	-0.003
	(.386)	(.387)	(.452)	(.46)	(.058)	(.367)	(.392)	(.384)	(.419)	(.063)
Hard roof	0.367	0.464	0.507	0.599	0.011	0.231	0.366	0.363	0.334	.159**
	(.484)	(.501)	(.501)	(.491)	(.078)	(.423)	(.484)	(.482)	(.472)	(.078)
Hard wall	0.518	0.573	0.575	0.581	0.052	0.402	0.357	0.393	0.391	-0.037
	(.501)	(.497)	(.495)	(.494)	(.071)	(.492)	(.481)	(.489)	(.489)	(.079)
Hard floor	0.831	0.827	0.845	0.933	-.087*	0.805	0.866	0.774	0.816	0.041
	(.376)	(.38)	(.363)	(.25)	(.05)	(.398)	(.342)	(.419)	(.388)	(.058)
Have automatic toilet	0.048	0.055	0.053	0.052	0.006	0.012	0.036	0.009	0	0.033
	(.215)	(.228)	(.224)	(.223)	(.035)	(.108)	(.186)	(.094)	(0)	(.024)
Have pit toilet	0.102	0.118	0.129	0.134	0.011	0.083	0.17	0.113	0.096	.097*
	(.304)	(.324)	(.336)	(.341)	(.056)	(.276)	(.377)	(.317)	(.295)	(.055)
Electricity	0.235	0.227	0.232	0.224	-0.002	0.166	0.179	0.161	0.142	0.02
	(.425)	(.421)	(.423)	(.417)	(.078)	(.373)	(.385)	(.368)	(.349)	(.051)
Piped water	0.03	0.045	0.041	0.047	0.009	0.006	0.027	0.018	0.011	0.026
	(.171)	(.209)	(.199)	(.211)	(.031)	(.077)	(.162)	(.133)	(.106)	(.018)
Poverty Index (0-292)	234.56	233.855	227.32	220.041	5.294	251.503	248.875	249.854	247.7	-1.834
	(39.843)	(39.426)	(43.884)	(45.032)	(6.966)	(24.17)	(26.574)	(27.738)	(27.71)	(4.551)
Test score (0-25)	19.855	19.791	19.821	19.93	-0.226	17.929	17.973	17.869	18.827	-1.095
	(3.281)	(3.454)	(3.15)	(2.851)	(.535)	(4.501)	(5.16)	(4.65)	(4.745)	(.817)
Observations	166	110	341	344	961	169	112	336	353	970
Attrition rate					0.287					0.29
Joint significance: Ho: all coef. =0										
Chi-square					26.09					24
p-value					0.05					0.09

Notes. All variables measured at baseline. Columns (1) to (4) (and (6) to (9), respectively) display the means for the control group attritors, the treatment group attritors, the control group surveyed and the treatment group surveyed. Standard deviations in parentheses. Column (5) and Column (10) provide the difference between the treatment group mean and the control group mean among attritors minus the difference between the treatment group mean and the control group mean among respondents. Differences in means are computed by OLS regression, controlling for province fixed effects. Standard errors in parentheses are clustered at the school level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The Chi-square (and corresponding p-value below) is the result of a test testing for the individual coefficients being jointly equal to 0 using seemingly unrelated estimation.

Table A2: Balance at Baseline, by Targeting Approach

	Merit Scholarship					Poverty Scholarship				
	n (1)	All (2)	Treatment (3)	Control (4)	Difference (5)	n (6)	All (7)	Treatment (8)	Control (9)	Difference (10)
Female	825	0.503	0.506	0.5	0.003	780	0.59	0.642	0.535	0.093***
		(0.5)	(0.501)	(0.501)	(0.035)		(0.492)	(0.48)	(0.499)	(0.035)
Number of minors	804	1.68	1.623	1.738	-0.132	765	1.822	1.861	1.782	0.081
		(1.109)	(1.091)	(1.124)	(0.123)		(1.084)	(1.046)	(1.122)	(0.119)
Own motorcycle	813	0.424	0.428	0.421	0.014	776	0.246	0.225	0.268	-0.024
		(0.495)	(0.495)	(0.494)	(0.054)		(0.431)	(0.418)	(0.443)	(0.042)
Own car/truck	799	0.148	0.171	0.124	0.028	780	0.037	0.048	0.026	0.013
		(0.355)	(0.377)	(0.33)	(0.035)		(0.189)	(0.215)	(0.159)	(0.022)
Own oxen/buffalo	817	0.558	0.581	0.535	0.014	776	0.441	0.5	0.38	0.065
		(0.497)	(0.494)	(0.499)	(0.056)		(0.497)	(0.501)	(0.486)	(0.051)
Own pig	822	0.585	0.609	0.562	0.031	776	0.488	0.545	0.431	0.079
		(0.493)	(0.489)	(0.497)	(0.049)		(0.5)	(0.499)	(0.496)	(0.055)
Own ox or buffalo cart	808	0.298	0.313	0.284	-0.001	767	0.211	0.244	0.177	0.032
		(0.458)	(0.464)	(0.451)	(0.045)		(0.408)	(0.43)	(0.382)	(0.045)
Hard roof	806	0.548	0.583	0.514	0.062	763	0.359	0.354	0.365	-0.024
		(0.498)	(0.494)	(0.5)	(0.05)		(0.48)	(0.479)	(0.482)	(0.052)
Hard wall	821	0.568	0.566	0.569	-0.011	776	0.403	0.416	0.39	0.011
		(0.496)	(0.496)	(0.496)	(0.052)		(0.491)	(0.494)	(0.488)	(0.061)
Hard floor	811	0.882	0.921	0.842	0.063*	774	0.801	0.818	0.783	0.002
		(0.323)	(0.27)	(0.365)	(0.033)		(0.399)	(0.386)	(0.413)	(0.047)
Have automatic toilet	804	0.052	0.055	0.049	0.003	775	0.008	0.003	0.013	-0.01
		(0.223)	(0.229)	(0.217)	(0.023)		(0.088)	(0.05)	(0.114)	(0.009)
Have pit toilet	804	0.132	0.131	0.133	0.005	775	0.112	0.116	0.108	0.018
		(0.339)	(0.337)	(0.34)	(0.039)		(0.316)	(0.321)	(0.311)	(0.034)
Electricity	822	0.232	0.227	0.238	-0.008	777	0.16	0.148	0.172	-0.022
		(0.423)	(0.419)	(0.426)	(0.047)		(0.366)	(0.355)	(0.378)	(0.04)
Piped water	818	0.045	0.042	0.049	-0.005	774	0.019	0.015	0.023	-0.01
		(0.208)	(0.2)	(0.215)	(0.021)		(0.138)	(0.123)	(0.152)	(0.013)
Poverty Index (0-292)	855	211.519	204.589	218.63	-10.969	784	245.043	243.426	246.703	-0.5
		(60.088)	(66.272)	(52.127)	(8.114)		(31.622)	(32.419)	(30.736)	(4.345)
Test score (0-25)	855	19.773	19.818	19.727	0.205	784	18.221	18.627	17.804	1.002
		(3.068)	(2.867)	(3.263)	(0.472)		(4.735)	(4.754)	(4.685)	(0.667)
Joint significance: Ho: all coef. =0										
Chi-square					14.04					15.25
p-value					0.6					0.51

Notes. Column (1) and Column (6) present the number of observations in the analysis sample (excluding observations with imputed baseline information). Colums (2) to (4) (and (7) to (9), respectively) display the means for the full sample, the treatment group, and the control group, respectively. Standard deviations in parentheses. Column (5) and Column (10) provide the difference between the treatment group mean and the control group mean. Differences in means are computed by OLS regression, controlling for province fixed effects. Standard errors in parentheses are clustered at the school level. *** p<0.01, ** p<0.05, * p<0.1. The Chi-square (and corresponding p-value below) is the result of a test testing for the individual coefficients being jointly equal to 0 using seemingly unrelated estimation.

Table A3: Education Outcomes

	Highest grade completed	Completed primary	Received any formal education in 2011-2017	**Family index**
	(1)	(2)	(3)	**(4)**
Treatment	0.241**	0.080**	0.068**	**0.189*** **
	(0.114)	(0.032)	(0.028)	**(0.067)**
Observations	1,370	1,370	1,370	**1,370**
R-squared	0.110	0.105	0.087	**0.130**
F test	3.004	3.357	3.496	**4.058**
Covariates	Yes	Yes	Yes	**Yes**
Control mean	5.478	0.573	0.745	**0.039**

Notes. Estimated treatment effects. The dependent variable in column (1) is the highest grade the individual completed and is equal to -1 if the individual received no education, 0 if he only went to kindergarten and then ranges from 1 to 11 for Grade 1 to Grade 11. In column (2), the dependent variable is a dummy equal to 1 if the individual completed primary education. In column (3), the dependent variable is equal to 1 if the individual was enrolled in the formal education system during any of the years 2011 to 2016. In column (4), the family index is the inverse covariance matrix-weighted mean of the standardized dependent variables from the three previous columns following Anderson (2008). Treatment captures effects for students who received any treatment, under either scheme. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table A4: Cognitive Outcomes

	Math	Raven's	Forward Digit Span	Picture Recognition Vocabulary Test	**Family index**
	(1)	(2)	(3)	(4)	**(5)**
Treatment	0.062	0.091	-0.031	0.049	**0.050**
	(0.063)	(0.059)	(0.057)	(0.073)	**(0.065)**
Observations	1,370	1,370	1,369	1,370	**1,369**
R-squared	0.110	0.121	0.048	0.213	**0.150**
F test	6.558	8.035	2.320	9.674	**8.617**
Covariates	Yes	Yes	Yes	Yes	**Yes**
Control mean	0.023	0.007	0.010	0.031	**0.018**

Notes. Estimated treatment effects. The dependent variable in column (1) is the score on the mathematics computer adaptive test, computed using Item Response Theory (IRT) with a two parameter logistic (2PL) model, standardized. In column (2), the dependent variable is the score on the Raven's matrices test computed using IRT with a 2PL model, standardized. In column (3), the dependent variable is the standardized score on the digit span test using forward items only, standardized. In column (4), the dependent variable is the score on a Picture Recognition Vocabulary Test computed using IRT with a 2PL model, standardized. In column (5), the family index is the inverse covariance matrix-weighted mean of the standardized dependent variables from the four previous columns following Anderson (2008). Treatment captures effects for students who received any treatment, under either scheme. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table A5: Socioemotional Outcomes

	SDQ			**Big 5**					**Family index**
	Prosocial (1)	Internalizing (2)	Externalizing (3)	Openness (4)	Conscientiousness (5)	Extraversion (6)	Agreeableness (7)	Neuroticism (8)	**(9)**
Treatment	-0.001	-0.084	-0.007	0.030	-0.039	-0.035	-0.057	-0.110*	**0.063**
	(0.058)	(0.059)	(0.058)	(0.055)	(0.056)	(0.063)	(0.058)	(0.059)	**(0.064)**
Observations	1,367	1,368	1,368	1,368	1,369	1,368	1,368	1,370	**1,360**
R-squared	0.040	0.075	0.041	0.036	0.034	0.052	0.036	0.023	**0.042**
F-statistic	1.912	4.065	1.632	2.094	2.361	2.050	1.546	1.370	**2.984**
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	-0.014	-0.003	-0.004	0.009	0.001	0.016	0.000	0.022	**-0.003**

Notes. Estimated treatment effects. The dependent variable in column (1) is the score from 0 to 10 on the pro-social facet (the higher the score, the more pro-social) of the Strengths and Difficulties Questionnaire (SDQ), standardized. In column (2), the dependent variables is the score from 0 to 20 on the internalizing behavior facet (the higher the score, the more externalizing behavior problems) of the SDQ, standardized. In column (3), the dependent variables is the score from 0 to 20 on the externalizing behavior facet (the higher the score, the more externalizing behavior problems) of the SDQ, standardized. In columns (4) to (8), the dependent variables are the scores from 3 to 15 on the Openness, Conscientiousness, Extraversion, Agreeableness, and Neuroticism facets of the Big Five scale, standardized. In column (9), the family index is the inverse covariance matrix-weighted mean of the standardized dependent variables from the eight first columns following Anderson (2008) (scores from columns (2), (3), and (8) have been flipped beforehand). Treatment captures effects for students who received any treatment, under either scheme. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Effects by Program Type and Labeling Effects on Education Outcomes

	Effects by Program Type			Labeling Effects		
	Highest grade completed	Completed primary	Received any formal education in 2011-2017	Highest grade completed	Completed primary	Received any formal education in 2011-2017
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment (any)	0.253*	0.055	0.042	0.265	0.036	0.038
	(0.130)	(0.040)	(0.031)	(0.173)	(0.054)	(0.042)
Treatment (poverty school)	-0.026	0.052	0.055	-0.034	0.054	0.028
	(0.149)	(0.048)	(0.039)	(0.160)	(0.055)	(0.043)
Treatment (merit school and non-poor)				-0.027	0.035	-0.017
				(0.220)	(0.067)	(0.051)
Treatment (poverty school and low scores)				0.002	0.036	0.097*
				(0.190)	(0.067)	(0.055)
Observations	1,370	1,370	1,370	1,370	1,370	1,370
R-squared	0.110	0.107	0.089	0.110	0.107	0.091
Effect in Poverty Schools	0.227	0.107***	0.097**			
	(0.142)	(0.041)	(0.038)			
Effect in Poverty Schools, if high scores				0.231	0.090*	0.066
				(0.175)	(0.051)	(0.044)
F-statistic	3.104	3.597	3.709	2.988	3.457	3.769
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	5.478	0.573	0.745	5.530	0.595	0.744

Notes. Estimated treatment effects. Dependent variables as in Table A3. In the panel to the left (effects by program type), treatment (any) captures effects for students who received the treatment, under the merit-based scheme. Effect in Poverty Schools captures effects for students who received the treatment, under the poverty-based scheme. In the panel to the right (labeling effects), treatment (any) captures effects for students who received the treatment under the merit-based scheme, but would have also qualified under the poverty-based scheme. Effect in Poverty Schools, if high scores captures effects for students who received the treatment under the poverty-based scheme, but would have also qualified under the merit-based scheme. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table A7: Effects by Program Type and Labeling Effects on Cognitive Outcomes

	Effects by Program Type				Labeling Effects			
	Math	Raven's	Forward Digit Span	Picture Recognition Vocabulary Test	Math	Raven's	Forward Digit Span	Picture Recognition Vocabulary Test
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment (any)	0.089	0.139**	0.079	0.066	0.230**	0.192**	0.096	0.178
	(0.083)	(0.070)	(0.065)	(0.084)	(0.107)	(0.095)	(0.089)	(0.115)
Treatment (poverty school)	-0.057	-0.102	-0.232***	-0.035	-0.139	-0.103	-0.214**	-0.053
	(0.092)	(0.089)	(0.082)	(0.101)	(0.105)	(0.098)	(0.096)	(0.114)
Treatment (merit school and non-poor)					-0.315**	-0.092	-0.012	-0.207
					(0.138)	(0.126)	(0.148)	(0.138)
Treatment (poverty school and low scores)					-0.027	-0.106	-0.096	-0.178
					(0.140)	(0.149)	(0.146)	(0.139)
Observations	1,370	1,370	1,369	1,370	1,370	1,370	1,369	1,370
R-squared	0.111	0.122	0.054	0.214	0.114	0.123	0.054	0.216
Effect in Poverty Schools	0.032	0.038	-0.153**	0.031				
	(0.072)	(0.077)	(0.075)	(0.094)				
Effect in Poverty Schools, if high scores					0.091	0.088	-0.118	0.125
					(0.095)	(0.093)	(0.096)	(0.118)
F-statistic	6.441	7.981	2.421	9.493	6.128	7.803	2.341	9.093
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.023	0.007	0.010	0.031	0.005	0.038	0.043	0.009

Notes. Estimated treatment effects. Dependent variables as in Table A4. In the panel to the left (effects by program type), treatment (any) captures effects for students who received the treatment, under the merit-based scheme. Effect in Poverty Schools captures effects for students who received the treatment, under the poverty-based scheme. In the panel to the right (labeling effects), treatment (any) captures effects for students who received the treatment under the merit-based scheme, but would have also qualified under the poverty-based scheme. Effect in Poverty Schools, if high scores captures effects for students who received the treatment under the poverty-based scheme, but would have also qualified under the merit-based scheme. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table A8: Effects by Program Type and Labeling Effects on Emotional and Behavioral Difficulties

	Effects by Program Type			**Labeling Effects**		
	Prosocial	Internalizing	Externalizing	Prosocial	Internalizing	Externalizing
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment (any)	-0.011	-0.109*	-0.026	0.137	-0.036	-0.007
	(0.066)	(0.064)	(0.076)	(0.096)	(0.099)	(0.100)
Treatment (poverty school)	0.022	0.054	0.039	0.025	0.033	-0.024
	(0.080)	(0.083)	(0.091)	(0.092)	(0.098)	(0.102)
Treatment (merit school and non-poor)				-0.253	-0.144	-0.089
				(0.157)	(0.147)	(0.134)
Treatment (poverty school and low scores)				-0.326**	-0.087	0.168
				(0.144)	(0.164)	(0.158)
Observations	1,367	1,368	1,368	1,367	1,368	1,368
R-squared	0.040	0.075	0.041	0.045	0.076	0.042
Effect in Poverty Schools	0.010	-0.055	0.013			
	(0.075)	(0.079)	(0.071)			
Effect in Poverty Schools, if high scores				0.162*	-0.003	-0.031
				(0.093)	(0.106)	(0.101)
F-statistic	1.882	4.036	1.594	2.062	4.070	1.581
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	-0.014	-0.003	-0.004	-0.137	0.010	0.007

Notes. Estimated treatment effects. Dependent variables as in Table A5. In the panel to the left (effects by program type), treatment (any) captures effects for students who received the treatment, under the merit-based scheme. Effect in Poverty Schools captures effects for students who received the treatment, under the poverty-based scheme. In the panel to the right (labeling effects), treatment (any) captures effects for students who received the treatment under the merit-based scheme, but would have also qualified under the poverty-based scheme. Effect in Poverty Schools, if high scores captures effects for students who received the treatment under the poverty-based scheme, but would have also qualified under the merit-based scheme. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A9: Effects by Program Type and Labeling Effects on Personality Traits

	Effects by Program Type					Labeling Effects				
	Openness (1)	Conscientiousness (2)	Extraversion (3)	Agreeableness (4)	Neuroticism (5)	Openness (6)	Conscientiousness (7)	Extraversion (8)	Agreeableness (9)	Neuroticism (10)
Treatment (any)	0.029	-0.072	0.035	-0.054	-0.066	0.132	-0.018	0.047	0.013	-0.045
	(0.067)	(0.065)	(0.076)	(0.068)	(0.073)	(0.103)	(0.087)	(0.108)	(0.090)	(0.102)
Treatment (poverty school)	0.002	0.069	-0.148	-0.007	-0.092	-0.035	0.110	-0.130	-0.044	-0.111
	(0.079)	(0.075)	(0.090)	(0.077)	(0.083)	(0.089)	(0.086)	(0.109)	(0.090)	(0.096)
Treatment (merit school and non-poor)						-0.211	-0.056	-0.005	-0.147	-0.052
						(0.157)	(0.145)	(0.147)	(0.136)	(0.138)
Treatment (poverty school and low scores)						-0.098	-0.250*	-0.083	-0.022	0.019
						(0.163)	(0.150)	(0.168)	(0.161)	(0.182)
Observations	1,368	1,369	1,368	1,368	1,370	1,368	1,369	1,368	1,368	1,370
R-squared	0.036	0.034	0.054	0.036	0.024	0.038	0.036	0.054	0.036	0.024
Effect in Poverty Schools	0.031	-0.003	-0.113	-0.061	-0.158**					
	0.068	0.071	0.078	0.071	0.071					
Effect in Poverty Schools, if high scores						0.097	0.092	-0.083	-0.031	-0.156
	(0.091)	(0.086)	(0.109)	(0.085)	(0.102)	0.091	0.086	0.109	0.085	0.102
F-statistic	2.048	2.481	2.031	1.541	1.366	2.159	2.346	1.948	1.503	1.323
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.009	0.001	0.016	0.000	0.022	-0.044	-0.030	-0.032	0.014	0.029

Notes. Estimated treatment effects. Dependent variables as in Table A5. In the panel to the left (effects by program type), treatment (any) captures effects for students who received the treatment, under the merit-based scheme. Effect in Poverty Schools captures effects for students who received the treatment, under the poverty-based scheme. In the panel to the right (labeling effects), treatment (any) captures effects for students who received the treatment under the merit-based scheme, but would have also qualified under the poverty-based scheme. Effect in Poverty Schools, if high scores captures effects for students who received the treatment under the poverty-based scheme, but would have also qualified under the merit-based scheme. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table A10: Labor Outcomes

	Currently working	Age started working	Any training since 2011	Cog. demands of main work (1/0)	Yearly earnings (inv. hyperbolic sine, USD)	Daily res. wage (inv. hyperbolic sine, USD)	**Family index**
	(1)	(2)	(3)	(4)	(5)	(6)	**(7)**
Treatment	0.026*	0.172	-0.030	-0.014	-0.216	0.044	**0.025**
	(0.015)	(0.178)	(0.030)	(0.022)	(0.162)	(0.037)	**(0.053)**
Observations	1,300	1,327	1,370	1,299	1,334	1,353	**1,241**
R-squared	0.062	0.040	0.092	0.056		0.071	**0.104**
F-statistic	2.530	1.857	4.435	2.226	2.485	2.029	**4.210**
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	**Yes**
Control mean	0.919	12.637	0.588	0.159	7.461	2.222	**-0.001**

Notes. Estimated treatment effects. The dependent variable in column (1) is a dummy equal to 1 if the individual is currently working, i.e. she worked for at least 1 hour during the last week or has a job at the moment but did not work during the last week. In column (2), the dependent variable is the age at which the individual started to work. In column (3), the dependent variable is a dummy equal to 1 if the individual participated in any formal or informal training that lasted at least one week, since 2011. In column (4), the dependent variable is a dummy equal to 1 if the main work activity demands cognitive ability (read, write, calculate, or use a computer) and 0 otherwise. In column (5), the dependent variable is the yearly earning expressed in US dollars and transformed using an inverse hyperbolic sine. In column (6), the dependent variable is the daily reservation wage in US dollars and transformed using an inverse hyperbolic sine. Treatment captures effects for students who received any treatment, under either scheme. In column (4), values for respondents who did not work have been imputed with 0, except if they were students. In columns (1) and (4), the sample is restricted to respondents who are not currently students. Column (2) includes everyone who ever worked. Column (4) includes only people who worked over the past 12 months. Columns (3) and (6) include the entire sample. In column (7), the family index is the inverse covariance matrix-weighted mean of the standardized dependent variables from the six first columns following Anderson (2008). Column (1), (2), (3), (4), (6), and (7) are estimated using OLS regression; column (5) is estimated using Tobit regression. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table A11: Well-being Outcomes

	SES Ladder (village) (1)	SES Index (IRT) (2)	Life Satisfaction (3)	Quality of Health (4)	Quality of Life (5)	Health Issue Index (GHQ) (6)	**Family index** **(7)**
Treatment	0.176***	0.061	0.079	0.130**	0.090*	-0.046	**0.159*** **
	(0.060)	(0.064)	(0.052)	(0.054)	(0.053)	(0.058)	**(0.055)**
Observations	1,370	1,370	1,369	1,370	1,370	1,355	**1,354**
R-squared	0.072	0.217	0.075	0.049	0.052	0.057	**0.109**
F-statistic	3.960	10.881	3.497	2.205	2.743	4.075	**6.207**
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	**Yes**
Control mean	-0.014	0.002	-0.020	0.008	0.012	-0.011	**-0.002**

Notes. Estimated treatment effects. The dependent variable in column (1) is the score from 1 to 10 on an economic ladder as compared to people of the same age in the village, standardized. In column (2), the dependent variables is a socio-economic index constructed based on asset ownership computed using Item Response Theory with a two parameter logistic model, standardized. In column (3), the dependent variable is the score from 1 to 10 on a life satisfaction question, standardized. In column (4), the dependent variable is the score from 1 to 5 on a health quality question, standardized. In column (5), the dependent variable is the score from 1 to 5 on a life quality question, standardized. In column (6), the dependent variable is the standardized score on the General Health Questionnaire. In column (7), the family index is the inverse covariance matrix-weighted mean of the standardized dependent variables from all the previous columns following Anderson (2008). Treatment captures effects for students who received any treatment, under either scheme. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table A12: Effects by Program Type and Labeling Effects on Labor Outcomes

	Effects by Program Type						Labeling Effects					
	Currently working	Age started working	Any training since 2011	Cog. demands of main work (1/0)	Yearly earnings (inv. hyperbolic sine, USD)	Daily res. wage (inv. hyperbolic sine, USD)	Currently working	Age started working	Any training since 2011	Cog. demands of main work (1/0)	Yearly earnings (inv. hyperbolic sine, USD)	Daily res. wage (inv. hyperbolic sine, USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment (any)	0.028	0.047	-0.024	-0.029	-0.214	0.068	0.016	0.304	-0.019	-0.011	-0.469*	0.097*
	(0.020)	(0.214)	(0.040)	(0.025)	(0.188)	(0.042)	(0.029)	(0.273)	(0.050)	(0.034)	(0.277)	(0.051)
Treatment (poverty school)	-0.003	0.263	-0.012	0.030	-0.004	-0.050	0.007	0.176	-0.004	0.000	0.107	-0.090
	(0.023)	(0.250)	(0.046)	(0.030)	(0.260)	(0.048)	(0.028)	(0.271)	(0.052)	(0.034)	(0.301)	(0.055)
Treatment (merit school and non-poor)							0.029	-0.515	-0.002	-0.055	0.537	-0.085
							(0.038)	(0.410)	(0.066)	(0.050)	(0.388)	(0.075)
Treatment (poverty school and low scores)							-0.009	-0.259	-0.039	0.059	0.176	0.069
							(0.042)	(0.484)	(0.071)	(0.052)	(0.412)	(0.093)
Observations	1,300	1,327	1,370	1,299	1,334	1,353	1,300	1,327	1,370	1,299	1,334	1,353
R-squared	0.062	0.041	0.092	0.056		0.072	0.063	0.042	0.092	0.059		0.073
Effect in Poverty Schools	0.025	0.310	-0.036	0.001	-0.217	0.018						
	(0.018)	(0.222)	(0.036)	(0.027)	(0.227)	(0.047)						
Effect in Poverty Schools, if high scores							0.023	0.480*	-0.022	-0.010	-0.362	0.008
							(0.029)	(0.281)	(0.047)	(0.034)	(0.261)	(0.057)
F-statistic	2.465	1.895	4.393	2.341	2.445	2.058	2.423	1.941	4.230	2.410	2.372	1.993
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.919	12.637	0.588	0.159	7.461	2.222	0.907	12.565	0.567	0.155	7.603	2.213

Notes. Estimated treatment effects. Dependent variables as in Table A10. In the panel to the left (effects by program type), treatment (any) captures effects for students who received the treatment, under the merit-based scheme. Effect in Poverty Schools captures effects for students who received the treatment, under the poverty-based scheme. In the panel to the right (labeling effects), treatment (any) captures effects for students who received the treatment under the merit-based scheme, but would have also qualified under the poverty-based scheme. Effect in Poverty Schools, if high scores captures effects for students who received the treatment under the poverty-based scheme, but would have also qualified under the merit-based scheme. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table A13: Effects by Program Type and Labeling Effects on Well-being Outcomes

	Effects by Program Type						Labeling Effects					
	SES Ladder (village)	SES Index (IRT)	Life Satisfaction	Quality of Health	Quality of Life	Health Issue Index (GHQ)	SES Ladder (village)	SES Index (IRT)	Life Satisfaction	Quality of Health	Quality of Life	Health Issue Index (GHQ)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment (any)	0.210***	0.159**	0.108	0.201***	0.076	-0.068	0.212**	0.180*	0.080	0.210**	0.061	0.025
	(0.077)	(0.076)	(0.067)	(0.066)	(0.062)	(0.069)	(0.101)	(0.093)	(0.092)	(0.098)	(0.090)	(0.090)
Treatment (poverty school)	-0.072	-0.204**	-0.059	-0.149*	0.031	0.047	-0.136	-0.199**	-0.043	-0.193*	0.034	-0.005
	(0.085)	(0.080)	(0.076)	(0.086)	(0.078)	(0.086)	(0.095)	(0.085)	(0.087)	(0.099)	(0.091)	(0.097)
Treatment (merit school and non-poor)							-0.060	-0.033	0.062	-0.056	0.028	-0.207
							(0.134)	(0.120)	(0.124)	(0.142)	(0.127)	(0.126)
Treatment (poverty school and low scores)							0.209	-0.064	0.005	0.126	0.019	-0.026
							(0.154)	(0.155)	(0.159)	(0.160)	(0.159)	(0.167)
Observations	1,370	1,370	1,369	1,370	1,370	1,355	1,370	1,370	1,369	1,370	1,370	1,355
R-squared	0.073	0.221	0.075	0.051	0.052	0.057	0.074	0.221	0.076	0.052	0.052	0.059
Effect in Poverty Schools	0.138**	-0.046	0.048	0.052	0.107	-0.021						
	(0.070)	(0.073)	(0.062)	(0.071)	(0.070)	(0.075)						
Effect in Poverty Schools, if high scores							0.076	-0.019	0.037	0.017	0.096	0.020
							(0.093)	(0.087)	(0.086)	(0.090)	(0.085)	(0.097)
F-statistic	3.880	10.670	3.551	2.353	2.691	4.063	3.927	10.405	3.409	2.275	2.597	4.119
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	-0.014	0.002	-0.020	0.008	0.012	-0.011	-0.065	-0.088	-0.088	0.003	-0.052	-0.018

Notes. Estimated treatment effects. Dependent variables as in Table A11. In the panel to the left (effects by program type), treatment (any) captures effects for students who received the treatment, under the merit-based scheme. Effect in Poverty Schools captures effects for students who received the treatment, under the poverty-based scheme. In the panel to the right (labeling effects), treatment (any) captures effects for students who received the treatment under the merit-based scheme, but would have also qualified under the poverty-based scheme. Effect in Poverty Schools, if high scores captures effects for students who received the treatment under the poverty-based scheme, but would have also qualified under the merit-based scheme. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table A14: Sensitivity to Differential Attrition (Effects by Program Type)

	Family index: Education (1)	Family index: Cognition (2)	Family index: Socioemotional (3)	Family index: Labor outcomes (4)	Family index: SES/Well-being (5)
Behaghel et al. (2014): Number of days tracking in 2017					
Lower bound					
Treatment (any)	0.133*	0.097	0.066	0.041	0.251***
	(0.080)	(0.078)	(0.081)	(0.062)	(0.073)
Treatment (poverty school)	0.079	-0.175*	0.002	-0.046	-0.153
	(0.102)	(0.096)	(0.100)	(0.084)	(0.096)
Effect in Poverty Schools	0.213**	-0.078	0.069	-0.004	0.098
	(0.091)	(0.084)	(0.084)	(0.075)	(0.076)
Upper bound					
Treatment (any)	0.134*	0.100	0.070	0.042	0.255***
	(0.079)	(0.078)	(0.081)	(0.062)	(0.073)
Treatment (poverty school)	0.083	-0.173*	0.002	-0.042	-0.153
	(0.102)	(0.096)	(0.100)	(0.084)	(0.096)
Effect in Poverty Schools	0.215**	-0.073	0.072	-0.001	0.103
	(0.091)	(0.085)	(0.084)	(0.075)	(0.076)
Behaghel et al. (2014): Probability of attrition based on 2011 tracking and attrition					
Treatment (any)	0.175**	0.154*	0.093	0.047	0.219***
	(0.077)	(0.078)	(0.083)	(0.064)	(0.073)
Treatment (poverty school)	0.113	-0.188**	-0.081	-0.051	-0.130
	(0.098)	(0.095)	(0.101)	(0.085)	(0.095)
Effect in Poverty Schools	0.287***	-0.034	0.012	-0.004	0.089
	(0.092)	(0.085)	(0.088)	(0.075)	(0.075)
Behaghel et al. (2014): Probability of attrition based on 2011 tracking and attrition, 2017 tracking					
Treatment (any)	0.144*	0.139*	0.070	0.037	0.234***
	(0.078)	(0.078)	(0.078)	(0.063)	(0.071)
Treatment (poverty school)	0.099	-0.184**	-0.030	-0.028	-0.150
	(0.099)	(0.093)	(0.094)	(0.081)	(0.091)
Effect in Poverty Schools	0.243***	-0.045	0.040	0.008	0.084
	(0.089)	(0.082)	(0.082)	(0.071)	(0.073)
IPW: Baseline covariates					
Treatment (any)	0.142*	0.128	0.069	0.029	0.231***
	(0.077)	(0.079)	(0.078)	(0.063)	(0.069)
Treatment (poverty school)	0.090	-0.156*	-0.005	0.004	-0.158*
	(0.096)	(0.094)	(0.095)	(0.078)	(0.087)
Effect in Poverty Schools	0.232***	-0.028	0.064	0.033	0.073
	(0.088)	(0.083)	(0.081)	(0.070)	(0.071)
IPW: 2011 tracking and attrition					
Treatment (any)	0.060	0.074	0.125	0.139	0.252***
	(0.100)	(0.093)	(0.097)	(0.084)	(0.077)
Treatment (poverty school)	0.242**	-0.157	0.018	0.064	-0.155*
	(0.113)	(0.109)	(0.116)	(0.097)	(0.092)
Effect in Poverty Schools	0.302***	-0.083	0.142	0.202**	0.097
	(0.094)	(0.102)	(0.100)	(0.095)	(0.080)
IPW: Baseline covariates, 2011 tracking and attrition, 2017 tracking					
Treatment (any)	-0.004	0.087	0.274**	0.145	0.234***
	(0.119)	(0.114)	(0.108)	(0.105)	(0.090)
Treatment (poverty school)	0.362***	-0.033	-0.116	0.111	-0.227**
	(0.125)	(0.125)	(0.117)	(0.121)	(0.111)
Effect in Poverty Schools	0.358***	0.054	0.159	0.256**	0.007
	(0.112)	(0.115)	(0.115)	(0.128)	(0.094)

Notes. Estimated treatment effects. The dependent variables in columns (1) to (5) are the family indices from Tables 3 and 4. Treatment (any) captures effects for students who received the treatment, under the merit-based scheme. Effect in Poverty Schools captures effects for students who received the treatment, under the poverty-based scheme. Behaghel et al. (2014) estimates tightened within three eligibility categories measured at baseline (high-performing and less poor, low-performing and poor, and high-performing and poor). Probability of attrition estimated through a probit regression of an attrition indicator on treatment status and covariates. "Baseline covariates" as listed in Table 1. "2011 tracking" information includes a 2011 indicator of attrition, the number of days needed for tracking, and its quadratic. "2017 tracking" refers to the study's five stages of respondent tracking, following Molina Millán and Macours (2017). All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table A15: Sensitivity to Differential Attrition (Labeling Effects)

	Family index: Education (1)	Family index: Cognition (2)	Family index: Socio-emotional (3)	Family index: Labor outcomes (4)	Family index: SES/Well-being (5)
Behaghel et al. (2014): Number of days tracking in 2017					
Lower bound					
Treatment (any)	0.109	0.192*	0.096	0.130	0.185*
	(0.109)	(0.103)	(0.116)	(0.093)	(0.096)
Treatment (poverty school)	0.030	-0.200*	0.021	-0.122	-0.158
	(0.117)	(0.108)	(0.120)	(0.101)	(0.108)
Effect in Poverty Schools	0.142	-0.008	0.119	0.009	0.027
	(0.108)	(0.108)	(0.115)	(0.094)	(0.093)
Upper bound					
Treatment (any)	0.111	0.198*	0.103	0.131	0.193**
	(0.109)	(0.103)	(0.117)	(0.093)	(0.096)
Treatment (poverty school)	0.033	-0.198*	0.023	-0.117	-0.158
	(0.116)	(0.108)	(0.120)	(0.100)	(0.107)
Effect in Poverty Schools, if high scores	0.144	0.000	0.124	0.013	0.035
	(0.108)	(0.108)	(0.115)	(0.094)	(0.094)
Behaghel et al. (2014): Probability of attrition based on 2011 tracking and attrition					
Treatment (any)	0.169	0.277***	0.161	0.151	0.155
	(0.107)	(0.103)	(0.121)	(0.097)	(0.096)
Treatment (poverty school)	0.066	-0.235**	-0.081	-0.107	-0.115
	(0.111)	(0.107)	(0.120)	(0.102)	(0.107)
Effect in Poverty Schools, if high scores	0.235**	0.042	0.080	0.044	0.040
	(0.110)	(0.105)	(0.114)	(0.095)	(0.093)
Behaghel et al. (2014): Probability of attrition based on 2011 tracking and attrition, 2017 tracking					
Treatment (any)	0.123	0.240**	0.122	0.138	0.189**
	(0.107)	(0.104)	(0.115)	(0.092)	(0.093)
Treatment (poverty school)	0.062	-0.223**	-0.008	-0.103	-0.153
	(0.113)	(0.106)	(0.113)	(0.098)	(0.103)
Effect in Poverty Schools, if high scores	0.185*	0.017	0.114	0.036	0.036
	(0.107)	(0.104)	(0.110)	(0.093)	(0.091)
IPW: Baseline covariates					
Treatment (any)	0.127	0.235**	0.124	0.134	0.187**
	(0.103)	(0.105)	(0.112)	(0.089)	(0.089)
Treatment (poverty school)	0.044	-0.193*	0.021	-0.083	-0.153
	(0.108)	(0.107)	(0.112)	(0.093)	(0.098)
Effect in Poverty Schools, if high scores	0.171	0.041	0.144	0.052	0.035
	(0.105)	(0.104)	(0.109)	(0.090)	(0.088)
IPW: 2011 tracking and attrition					
Treatment (any)	0.043	0.178	0.222	0.316**	0.286***
	(0.147)	(0.117)	(0.157)	(0.128)	(0.093)
Treatment (poverty school)	0.179	-0.200	0.041	-0.012	-0.122
	(0.141)	(0.122)	(0.148)	(0.103)	(0.096)
Effect in Poverty Schools, if high scores	0.223*	-0.022	0.263*	0.304**	0.164
	(0.121)	(0.127)	(0.139)	(0.139)	(0.101)
IPW: Baseline covariates, 2011 tracking and attrition, 2017 tracking					
Treatment (any)	0.183	0.207	0.347**	0.385**	0.303***
	(0.154)	(0.144)	(0.160)	(0.185)	(0.109)
Treatment (poverty school)	0.174	-0.054	0.017	0.090	-0.183*
	(0.142)	(0.135)	(0.142)	(0.122)	(0.102)
Effect in Poverty Schools, if high scores	0.357**	0.153	0.364**	0.474**	0.121
	(0.149)	(0.151)	(0.164)	(0.204)	(0.107)

Notes. Estimated treatment effects. The dependent variables in columns (1) to (5) are the family indices from Tables 3 and 4. Treatment (any) captures effects for students who received the treatment under the merit-based scheme, but would have also qualified under the poverty-based scheme. Effect in Poverty Schools, if high scores captures effects for students who received the treatment under the poverty-based scheme, but would have also qualified under the merit-based scheme. Behaghel et al. (2014) estimates tightened within three eligibility categories measured at baseline (high-performing and less poor, low-performing and poor, and high-performing and poor). Probability of attrition estimated through a probit regression of an attrition indicator on treatment status and covariates. "Baseline covariates" as listed in Table 1. "2011 tracking" information includes a 2011 indicator of attrition, the number of days needed for tracking, and its quadratic. "2017 tracking" refers to the study's five stages of respondent tracking, following Molina Millán and Macours (2017). All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table A16: Robustness of three-year results to attrition after nine years

	Completed primary	Highest grade completed	Math	Forward Digit Span
	(1)	(2)	(3)	(4)
Treatment (any)	0.113***	0.151	0.181*	0.126*
	(0.041)	(0.101)	(0.097)	(0.070)
Treatment (poverty school)	0.080*	0.159	-0.161	-0.158
	(0.048)	(0.120)	(0.107)	(0.096)
Treatment (merit school and non-poor)				
Treatment (poverty school and low scores)				
Observations	1,259	1,259	1,323	1,323
R-squared	0.144	0.095	0.146	0.073
Effect in Poverty Schools	0.193***	0.310**	0.020	-0.031
	(0.043)	(0.122)	(0.083)	(0.085)
F-statistic	3.259	2.032	6.023	2.572
Covariates	Yes	Yes	Yes	Yes
Control mean	0.603	5.415	0.021	0.008

Notes. Estimated treatment effects after three years. This table replicates the main findings reported on in Author 1 and Author 3 (2016), with the (pooled) sample of individuals who did not attrit over nine years. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table A17: Heterogeneous Treatment Effects by Gender: Education Outcomes

	Highest grade completed	Completed primary	Received any formal education in 2011-2017
	(1)	(2)	(3)
Treatment	0.199	0.068	0.053
	(0.129)	(0.043)	(0.035)
Female and treatment	0.069	0.016	0.028
	(0.175)	(0.061)	(0.044)
Female	-0.120	0.014	-0.055
	(0.178)	(0.054)	(0.043)
Observations	1,337	1,337	1,337
R-squared	0.114	0.108	0.088
Effect for females	0.268*	0.084*	0.081**
	(0.155)	(0.045)	(0.037)
F-statistic	3.243	3.416	3.513
Covariates	Yes	Yes	Yes
Control mean (females)	5.385	0.565	0.723

Notes. Estimated treatment effects. Dependent variables as in Table A3. Treatment captures effects for male students who received any treatment, under either scheme. Effect for females captures the respective effect for female students. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table A18: Heterogeneous Treatment Effects by Gender: Cognitive Outcomes

	Math	Raven's	Forward Digit Span	Picture Recognition Vocabulary Test
	(1)	(2)	(3)	(4)
Treatment	0.088	0.193**	0.084	0.211**
	(0.089)	(0.086)	(0.085)	(0.096)
Female and treatment	-0.057	-0.205*	-0.211*	-0.307***
	(0.111)	(0.118)	(0.111)	(0.114)
Female	-0.182*	-0.261**	-0.003	-0.174
	(0.095)	(0.107)	(0.108)	(0.108)
Observations	1,337	1,337	1,336	1,337
R-squared	0.112	0.124	0.050	0.219
Effect for females	0.031	-0.012	-0.127*	-0.096
	(0.081)	(0.081)	(0.076)	(0.089)
F-statistic	6.317	8.258	2.194	11.646
Covariates	Yes	Yes	Yes	Yes
Control mean (females)	-0.075	-0.136	0.006	-0.046

Notes. Estimated treatment effects. Dependent variables as in Table A4. Treatment captures effects for male students who received any treatment, under either scheme. Effect for females captures the respective effect for female students. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table A19: Heterogeneous Treatment Effects by Gender: Socioemotional Outcomes

	SDQ			**Big 5**				
	Prosocial (1)	Internalizing (2)	Externalizing (3)	Openness (4)	Conscientiousness (5)	Extraversion (6)	Agreeableness (7)	Neuroticism (8)
Treatment	0.028	-0.134	-0.020	-0.019	-0.040	0.068	-0.051	-0.102
	(0.077)	(0.087)	(0.088)	(0.084)	(0.085)	(0.086)	(0.088)	(0.082)
Female and treatment	-0.045	0.092	0.023	0.100	0.014	-0.178	-0.003	-0.000
	(0.103)	(0.115)	(0.120)	(0.114)	(0.116)	(0.120)	(0.116)	(0.114)
Female	0.098	0.234**	0.231**	-0.074	-0.134	0.278***	-0.043	-0.056
	(0.098)	(0.101)	(0.098)	(0.093)	(0.112)	(0.102)	(0.119)	(0.099)
Observations	1,334	1,336	1,335	1,335	1,336	1,335	1,335	1,337
R-squared	0.043	0.077	0.042	0.039	0.036	0.054	0.037	0.024
Effect for females	-0.017	-0.042	0.003	0.082	-0.026	-0.109	-0.053	-0.102
	(0.079)	(0.077)	(0.080)	(0.075)	(0.076)	(0.086)	(0.076)	(0.082)
F-statistic	2.081	4.568	1.658	2.054	2.261	1.874	1.793	1.606
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control mean	0.051	0.159	0.084	-0.038	-0.047	0.132	0.040	0.039

Notes. Estimated treatment effects. Dependent variables as in Table A5. Treatment captures effects for male students who received any treatment, under either scheme. Effect for females captures the respective effect for female students. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table A20: Heterogeneous Treatment Effects by Gender: Labor Outcomes

	Currently working	Age started working	Any training since 2011	Cog. demands of main work (1/0)	Yearly earnings (inv. hyperbolic sine, USD)	Daily res. wage (inv. hyperbolic sine, USD)
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.035**	0.271	-0.000	-0.007	-0.160	0.072
	(0.017)	(0.265)	(0.042)	(0.031)	(0.224)	(0.058)
Female and treatment	-0.018	-0.218	-0.053	-0.024	-0.131	-0.053
	(0.026)	(0.357)	(0.054)	(0.042)	(0.309)	(0.074)
Female	-0.045*	-0.334	-0.145***	0.022	-0.728**	-0.091
	(0.024)	(0.321)	(0.045)	(0.039)	(0.284)	(0.064)
Observations	1,267	1,295	1,337	1,267	1,301	1,320
R-squared	0.068	0.039	0.092	0.055		0.068
Effect for females	0.017	0.054	-0.053	-0.030	-0.291	0.019
	(0.022)	(0.241)	(0.040)	(0.030)	(0.223)	(0.047)
F-statistic	2.586	1.916	4.690	2.061	2.404	2.077
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Control mean (females)	0.891	12.449	0.511	0.167	7.265	2.178

Notes. Estimated treatment effects. Dependent variables as in Table A10. Treatment captures effects for male students who received any treatment, under either scheme. Effect for females captures the respective effect for female students. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Table A21: Heterogeneous Treatment Effects by Gender: Well-being Outcomes

	SES Ladder (village) (1)	SES Index (IRT) (2)	Life Satisfaction (3)	Quality of Health (4)	Quality of Life (5)	Health Issue Index (GHQ) (6)
Treatment	0.275***	0.176**	-0.007	0.256***	0.087	-0.006
	(0.078)	(0.080)	(0.079)	(0.077)	(0.079)	(0.080)
Female and treatment	-0.194*	-0.207*	0.153	-0.226*	0.002	-0.066
	(0.116)	(0.106)	(0.118)	(0.121)	(0.108)	(0.109)
Female	0.029	0.079	-0.240**	-0.063	-0.003	0.237**
	(0.100)	(0.084)	(0.115)	(0.095)	(0.097)	(0.099)
Observations	1,337	1,337	1,336	1,337	1,337	1,322
R-squared	0.077	0.221	0.077	0.053	0.052	0.054
Effect for females	0.080	-0.030	0.146*	0.030	0.089	-0.072
	(0.088)	(0.086)	(0.079)	(0.083)	(0.074)	(0.079)
F-statistic	4.216	10.516	3.559	2.378	2.835	3.255
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Control mean (females)	-0.016	-0.025	-0.172	-0.043	-0.038	0.122

Notes. Estimated treatment effects. Dependent variables as in Table A11. Treatment captures effects for male students who received any treatment, under either scheme. Effect for females captures the respective effect for female students. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A22: Heterogeneous Treatment Effects by Gender and Program-Type

	Family index: Education (1)	Family index: Cognition (2)	Family index: Socio-emotional (3)	Family index: Labor (4)	Family index: SES/Well-being (5)
A. Effects among male respondents					
Merit school	0.123	0.246**	0.079	0.026	0.264***
	(0.084)	(0.105)	(0.097)	(0.088)	(0.082)
Poverty school	0.191*	0.091	0.165	0.154	0.120
	(0.103)	(0.116)	(0.110)	(0.100)	(0.103)
Poverty vs. merit	0.068	-0.155	0.086	0.128	-0.145
	(0.109)	(0.119)	(0.121)	(0.111)	(0.113)
B. Effects among female respondents					
Merit school	0.136	0.008	0.076	0.011	0.195*
	(0.109)	(0.103)	(0.110)	(0.079)	(0.102)
Poverty school	0.283***	-0.138	-0.012	-0.088	0.048
	(0.105)	(0.097)	(0.093)	(0.087)	(0.096)
Poverty vs. merit	0.148	-0.146	-0.088	-0.099	-0.146
	(0.110)	(0.112)	(0.111)	(0.093)	(0.112)
C. Difference in effects (female vs. male)					
Merit school	0.013	-0.238*	-0.004	-0.015	-0.070
	(0.118)	(0.133)	(0.135)	(0.111)	(0.119)
Poverty school	0.092	-0.228*	-0.177	-0.242*	-0.072
	(0.116)	(0.136)	(0.123)	(0.125)	(0.141)
Observations	1,370	1,369	1,360	1,241	1,354

Notes. Estimated treatment effects. The dependent variables in columns (1) to (5) are the family indices from Tables 3 and 4. Each cell shows the linear combination of treatment effects for a given program type and gender or the difference between these treatment effects. All regressions control for province fixed effects, baseline test score, baseline poverty score, individual-level socio-economic variables from baseline, 6 school-level (EMIS) variables and 5 census village-level variables, measured at baseline. Standard errors are in parentheses (clustered at the school level). *** p<0.01, ** p<0.05, * p<0.1.

Appendix B Complementarities between Cognitive and Socio-emotional Outcomes

Our approach is based on two different conceptual models of the relationships between years of education (E), cognitive skills (C), and socio-emotional skills (S). As a starting point, based on the evaluation three years after the program's inception (Author 1 and Author 3 2016), we know that treatment T_0 (at baseline, $t = 0$) increased years of education schooling for both merit- and poverty-based scholarships ($E_t = f(T_0; X_0, Z_0)$; $\frac{E_t}{\partial T_0} > 0$, for both types of scholarships, where X_0 are student characteristics and Z_0 are school inputs). Furthermore, the evaluation showed a causal, positive effect of the intervention on cognitive skills for the merit-based scholarship only ($C_t^M = f(T_0^M; X_0, Z_0)$, $\frac{\partial C_t^M}{\partial T_0^M} > 0$); and zero effects for the poverty-based scholarship ($C_t^P = f(T_0^P; X_0, Z_0)$, $\frac{\partial C_t^P}{\partial T_0^P} = 0$), where M denotes merit-based treatment and P denotes poverty-based treatment.

The first conceptual relationship we explore is that between each type of skill—cognitive and socio-emotional—and years of education:

$$C_t = g(E_t; X_0, Z_0)$$
$$S_t = g(E_t; X_0, Z_0)$$

These equations state that the effect on either set of skills is a function of the years of education; i.e., exposure to more schooling will induce higher cognitive and socio-emotional skills. Therefore, the first set of relationships we investigate are:

$$\frac{\partial C}{\partial T} = \frac{\partial C}{\partial E} * \frac{\partial E}{\partial T} > 0 \tag{4}$$

and

$$\frac{\partial S}{\partial T} = \frac{\partial S}{\partial E} * \frac{\partial E}{\partial T} > 0 \tag{5}$$

If schooling produces cognitive and socio-emotional skills, both equations 4 and 5 are positive, independently of the type of treatment (merit or poverty).

In contrast, the second conceptual relationship is based on a modification of this setup: for the merit-based scholarship we have an additional equation, relating cognitive skills and treatment:

$$C_t^M = f(T_0^M) \tag{6}$$

i.e., treatment induced higher cognitive skills only for the merit (M) treatment. The basic relationship of interest is between socio-emotional skills and cognitive skills:

$$S_t^M = g(C_t^M, E_t; X_0, Z_0)$$

The second relationship we investigate is therefore:

$$\frac{\partial S_t^M}{\partial T_0^M} = \frac{\partial S_t^M}{\partial C_t^M} * \frac{\partial C_t^M}{\partial T_0^M} + \frac{\partial S_t^M}{\partial E_t} * \frac{\partial E_t}{\partial T_0^M} > 0 \tag{7}$$

i.e., that the effect of treatment on socio-emotional skills is positive, and it depends on the effect of cognitive skills on socio-emotional skills ($\frac{\partial S_t^M}{\partial C_t^M}$) and on the indirect effect of higher exposure to more schooling ($\frac{\partial S_t^M}{\partial E_t}$). If there is complementarity (or co-production) between cognitive and socio-emotional skills (i.e., $\frac{\partial S_t^M}{\partial C_t^M} > 0$), then $\frac{\partial S_t^M}{\partial T_0^M} > 0$.

For the case of the poverty-based scholarship (P), the corresponding expression is:

$$\frac{\partial S_t^P}{\partial T_0^P} = \frac{\partial S_t^P}{\partial E_t} * \frac{\partial E_t}{\partial T_0^P} \tag{8}$$

since

$$\frac{\partial C_t^P}{\partial T_0^P} = 0$$

. There are three main relevant cases for Equations 7 and 8. If exposure to school in-and-of itself produces socio-emotional skills, both Equation 7 and 8 are positive. If exposure to schooling does not produce socio-emotional skills, Equation 8 is equal to zero. Finally, under complementarities between cognitive and socio-emotional skills (e.g. if cognitive skills help in the acquisition of socio-emotional skills, or if they are co-produced), then Equation 7 is positive, independent of the relationship between socio-emotional skills and exposure to school.

Appendix C Returns to Schooling in Cambodia by Gender

There is no strong evidence that the Mincerian returns to education are different for males and females in Cambodia. Based on data from 2007, Lall and Sakellariou (2010) show that each year of education were associated with increased private sector earnings of 9 and 8 percent for males and females respectively (12 and 13 percent after correcting for selection using a Heckman correction approach). Nevertheless, the rural and remote context of our study might be different. Analysis of the rural sample of the 2012 Cambodia Labor Force and Child Labor Survey reveals similar results (see Table C1).[52] In these data, wages increase by 8.5 and 7 percent for each year of schooling among 19 to 24 year old male and female paid rural workers respectively, and by about 11.5 percent among 25 to 65 year old paid rural workers—with no discernible differences by gender.[53]

These data do, however, reveal some gender differences in schooling and labor force participation patterns. In particular, whereas 21 percent of 19 to 24-year-old females were not in school and out of the labor force, only 12 percent of males were (see Table C2). Among those who were working, patterns in the type of work are similar across genders among 19- to 24-year-old individuals; the most notable differences are that males were about 5 percentage points more likely to be doing paid work in agriculture (16.7 percent of males versus 11.6 percent of females) and males are about 5 percentage points more likely to be self-employed outside of agriculture (5.3 versus 9.0 percent for males and females respectively). In an older cohort (ages 25 to 39) males were about 10 percentage points more likely to be working for pay (mostly outside of agriculture—40.6 versus 28.5 percent) whereas females were more likely to be in unpaid or self-employed work in agriculture (29.9 versus 20.6 percent for the two categories combined).

[52] These data were extracted from The World Bank (2013).

[53] Further disaggregating the data to the three provinces included in our study yielded sample sizes that were too small to provide meaningful estimates.

These findings are suggestive that the rewards to schooling, and perhaps cognitive skills, might be lower for females in that they are more likely to be out of the labor force, and although the earnings increments to schooling are similar for males and females, the latter are more likely to be in unpaid or self-employed occupations. Education investments might therefore yield lower returns to girls, perhaps, in part, explaining the lower of increment in cognitive scores and other outcomes among girls associated with scholarships.

Table C1: Log Wage Regressions for Paid Workers

	Males 19-24 (1)	Females 19-24 (2)	Males 25-65 (3)	Females 25-65 (4)
Years of schooling	0.0847***	0.0704***	0.116***	0.115***
	(0.0102)	(0.00956)	(0.00800)	(0.00979)
Age	0.586	1.306**	0.247**	0.0865
	(0.591)	(0.560)	(0.120)	(0.138)
Age squared	-0.0128	-0.0299**	-0.00406**	-0.00199
	(0.0137)	(0.0131)	(0.00191)	(0.00222)
Constant	4.771	-2.374	7.531***	10.60***
	(6.325)	(5.960)	(1.862)	(2.124)
Observations	1,463	1,244	1,867	1,174
R-squared	0.051	0.048	0.107	0.135

Notes. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Sample. Rural Cambodia from the 2012 Cambodia Labor Force and Child Labor Survey.

Table C2: School and Work Patterns

	In school, not working	In school, working	Not in school, working	Not in school, unemployed	Not in school, out of labor force	Total
Male 10-14	77.4	12.5	4.7	0.0	5.3	100
Female 10-14	78.5	14.3	3.3	0.0	3.9	100
Male 15-18	36.7	13.0	40.1	0.7	9.5	100
Female 15-18	32.9	12.5	42.0	0.5	12.1	100
Male 19-24	12.6	3.9	70.3	1.0	12.3	100
Female 19-24	8.9	3.1	65.9	0.8	21.4	100
Total	39.47	9.41	39.52	0.53	11.07	100

Sample. Rural Cambodia from the 2012 Cambodia Labor Force and Child Labor Survey.